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ALEXANDER VON HUMBOLDT.

(From the portrait by Schrader, in possession of Albert Havemeyer, Esq., New York.)

In 1970 the A.A.G. Committee on Archives and Association History was established. Since that time the Committee has arranged numerous sessions and workshops concerning the history of geographic thought, facilitated the establishment of an official archival repository, supported Maynard Weston Dow's "Geographers on Film" project, advised on the U.S. bio-bibliography contribution to *Geographers*, and otherwise sought to foster and encourage study of geographical thought. The first number of the *History of Geography Newsletter* was published in December 1981. With this number, the publication is retitled, *History of Geography Journal*.

The purpose of the *Journal* is to provide an outlet for articles, summaries, reports of events, reviews or notices of literature et al. relating to the history of geographic thought. If you have something of this genre which you feel would be appropriately placed in this publication, please submit same to the editors. It is anticipated that this *Journal* will be published occasionally, but nevertheless not infrequently.

Klaus D. Gurgel joins with me in producing this number of the *Journal*. Recognition for help received in typesetting the manuscripts for this *Journal* is extended to Ruth L. Gurgel of the Atlas Publishing Company. We wish to thank Southern Connecticut State University for helping to defray the costs of producing this volume of the *Journal*.

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Hettner's Exceptionalism -- Fact or Fiction?*

Richard Hartshorne

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IN THINKING about this meeting and in driving here, I was reminded of my first visit to this university, thirty-three years ago. I came at the invitation of the department chairman, my good friend Harold McCarty, to give a public lecture, but more particularly, he told me, at the request of Fred Schaefer, who wished to have me meet with his seminar on the history of geographic thought.

I had met Schaefer once before, in 1946, at a meeting when we happened to be to ourselves at lunch -- the only occasion I can remember when just the two of us talked together. I believe he said something of his experiences in Germany under the Nazis, and I no doubt mentioned my experience in living in a Jewish household in Vienna, also under the Nazis. He spoke of the seminar he was giving on geographic thought; told me that he was using my work and had some questions he hoped I would be willing to discuss there. Of course I replied in the affirmative.

So in May 1950 I came here and we had two meetings. One was an evening meeting in his home with colleagues from other departments -- of whom Professor Bergmann was one, and I regret he is unable to be here tonight. My memory is one of tasteful cordials and cordial conversation.

I remember more clearly meeting most of a Saturday morning with Schaefer's seminar, in which he functioned as chairman or moderator while his students directed a barrage of questions to me concerning what I had written about geography. I was impressed with their knowledge of what I had written and their ability to present challenging questions. I enjoyed the challenge and hoped I was responding adequately. The letter Schaefer wrote me the following week more than reassured me, so I wish to read from it: "Let me express my and the students' heartfelt gratitude for that session. Naturally there was disagreement here and there. But everyone felt the same about your fortitude and splendid response to the questioning."

We did not happen to meet again, and had no further communication. I knew nothing of what he was working on, nor of his state of health, until his article appeared in the *Annals* in 1953, bringing also the news of his death.

That is the whole of my personal experience with or concerning Fred Schaefer. So I am confident that personal relations were in no way a cause for Schaefer's writing as he did about what I had written, and I knew that they were not a cause of my replying as I did to what he wrote. Rather, each of us was disturbed only by what the other had written, and each of us wrote about the other only in respect to what had disturbed him in what the other had written.

Schaefer, on his part, believed that certain views expressed in my *Nature of Geography* -- views based closely, though not solely, on the writing of Alfred Hettner -- that these views were obstacles preventing geography from developing as a science -- according to the concept of science he held to be the only true one. Believing that, he set out to attack our views, to demolish them as based on "fallacious" antecedents and "spurious" assumptions, leading to "nonscientific or antiscientific" consequences.

The ideas in question he said were based on a "basic fallacy" first stated by the philosopher Kant in the eighteenth century and imposed on geography by Hettner more than a century later. This declared that geography was like history, "methodologically different from all other sciences," or more baldly, "history and geography are separate disciplines outside the sciences." On this basis Hettner had deduced further similarities between geography and history not found in other sciences. For the whole group of ideas, Schaefer coined the term "exceptionalism."

This included in particular: 1) the task of integrating phenomena of the most diverse kinds; 2) a duality of regional and systematic geography in which regional geography is really the essential part; and 3) -- the issue of greatest concern -- "the typically romantic argument from uniqueness," as he called it -- the study of individual objects or places or areas rather than generic concepts and scientific laws -- in technical terms, the idiographic versus nomothetic approach.

The argument on this main issue cannot be stated simply because of the strange three-part structure of the presentation. First, Hettner's position is stated as solely idiographic: Because geography is like history, it deals with unique rather than generic cases; in place of explanations under scientific laws Hettner used a method of historicism which yields nothing. Among American geographers following Hettner's his-

* An invited paper read to a symposium on "Exceptionalism in Geography - Thirty Years After Schaefer," West Lakes AAG Meeting, Iowa City, November 3, 1983.

toricism, Carl Sauer is cited as perhaps the outstanding representative.

In the second part, my position is presented in similar terms: "Geography is essentially idiographic. Whenever laws are discovered or applied one is no longer in the area of geography. All it contributes is facts." Then the paper returns to Hettner to present his position in a very different light, by quoting passages from his writings that support the nomothetic position. This leads to a double-barrelled conclusion knocking out both targets: Hettner is found to have been confused and vacillating, advocating at different times and in different places the idiographic or the nomothetic conceptions; and I am found responsible for misleading American students by using quotations from Hettner that give only the idiographic side of his position. Current writings on this topic demonstrate that there are geographers who believe that.

In reacting to the Schaefer paper I was not disturbed by his concern that geography be recognized as a science. What disturbed me, as I stated in the first sentences of my response, were the large number of cases in which the views of Hettner and myself -- and numerous others -- seemed to me to be of doubtful reliability. I know now that I was not alone in this -- that such doubts had been raised to the author by critics before publication, but without effect. But I learned that many readers accepted the presentations at face value, overlooking the scarcity of definitive citations and the use of free paraphrasing devoid of precise references that would enable a reader to check for reliability.

Hence my response in the *Annals* of 1955 was written to examine the evidence presented in the original article in order to determine what could be accepted as historic fact, what was unreliable or definitely not true. In the nearly thirty years since that was published I have had no challenge on any item or on my quantification of the proportion of errors to truths. So I see no need to say more on that score.

What I wish to do now is to look at the positive side -- to consider the antecedents of Hettner's thinking and present what he *did* say on the issues listed under the term exceptionalism.

Throughout his methodological writings, Hettner emphasized that his views of geography had developed not by logical deduction from a priori assumptions or authoritative doctrine, but from study of its history -- what geographers did and what they thought about what they were doing. Tentative conclusions from that needed to be tested in substantive research, in the study and in the field. The construction of a logical justification was a final rather than initial step.

The evidence in his writings indicates clearly, I believe, that the antecedents of his thinking are to be found in the development of geographic thought in Germany in the nineteenth cen-

tury. Especially important were the discussions in the last quarter of the century by the generation of German geographers who were writing while he was a student.

The earlier sources most commonly cited in these discussions were the methodological writings of Ritter and Humboldt's substantive writings -- but not his methodological statements. Kant's name or ideas appear not to have been mentioned. Of those participating in the discussions, Richthofen and at least two others -- Marthe and Hermann Wagner -- clearly had significant influence on Hettner's thinking. Among the topics discussed were several that Schaefer included under his concept of exceptionalism. We will consider each of these briefly.

Many of Hettner's predecessors, coming to geography from the specialized natural or social sciences, were concerned over the wide range and variety of different kinds of phenomena that were included for study in geography. It was no wonder, Hettner noted in 1895, that many questioned whether such a diversity could be united in one discipline. One of his principal teachers, Gerland, had said no, it could not. Using Ritter's famous phrase in reverse, he declared, "A science of the heterogeneous things that fill area is impossible because of their heterogeneity;" geography, he urged, should exclude human features and so function as a natural science. Many who would not go that far accepted the concept that geography was "dualistic," divided into two halves -- physical and human geography.

Hettner, however, had noted in his early article (1898) on the development of geography in the nineteenth century that for both Humboldt and Ritter the distinctive feature of geography was to demonstrate the causal relations among all kinds of phenomena in an area. Man and nature, Hettner concluded, were intrinsically interwoven; while the inclusion of both admittedly made the study of geography more difficult, it was unavoidable. Heterogeneity, however, was not unique to geography, but was true in varying degree, Hettner noted, in history, prehistory, and paleontology. I found that William Morris Davis had noted the same for astronomy -- in lesser degree only because of the failure as yet to find evidence of life in the heavens!

Hettner also anticipated and refuted the possibility of reducing the multiplicity of things studied by limiting geography to an abstract *Raumwissenschaft*, concerned only with spatial forms and patterns.

Also called a "dualism" by some was the distinction between the method of approach in systematic and in regional geography. This, of course, dated from long before Hettner, or even Kant -- since as early as Varenus, in 1650, or Keckerman, in 1611. In the nineteenth century, and since, there have been marked swings in the relative value or importance of the

two approaches -- even arguments at one time that only one, and then later only the other, was the true geography.

Hettner, following Richthofen, explained the difference in functions of the two approaches and the ways in which they were interrelated. In doing that he at one time characterized regional geography as the "core" of geography. I also used that metaphor, or similarly, the "heart of geography," or, with Fenneman, "the center." And I had added Fenneman's note of caution: "There is no more worth in a center than in a border, no more dignity in one part of the field than another." In respect to the relative importance of the two approaches, however, Hettner time and again (together with Penck and others) insisted: The two approaches are of equal importance or value; they are by no means as distinct as often supposed, but are inter-connected in every part, intimately related, essential to each other and to geography as a whole.

The discussion of this topic in the Schaefer paper is surprisingly similar in thought and wording to what I had presented as Hettner's views. But they are presented without indication of source, and as though in opposition to the view of Hettner and myself. Concerning our view, the paper refers only to the figure of speech of the core or heart of geography, as though that could be taken as a definitive measure of relative importance.

I can find no suggestion in Hettner's discussions that the contrast between systematic and regional geography was unique to geography; I had pointed out clear parallels in history, astronomy, geology, and very strikingly in anatomy, where the two are called systematic and topographical.

A third issue frequently raised in discussions among geographers of that period -- and later -- concerned the standing of geography as a science. For many, then and now, the essential character of a science was the use of generic concepts each covering innumerable instances of highly similar things or events, concepts necessary for the construction of laws stating the consequences of interaction among phenomena.

Such methods had been developed in various branches of systematic **physical** geography, and used in regional geography. And Ratzel was promoting the same in human geography. But many studies in systematic and especially in regional geography focussed on a particular, individual case, place, or area, because it differed from any others in some significant respects. That is, it was seen as (in German) *einmalig*, a once-only event, place, or time or (literally the same in Latin-based English) -- unique.

Students of the philosophy of knowledge were also interested in this difference. One of them, Windelband, coined the term "nomothetic" for generic or generalizing studies, and "idiographic" for studies of individual cases. His follower Rickert endeavored to divide the fields of learned knowledge

on this basis. Hettner argued repeatedly against that idea as both illogical and impractical.

They did not, nor did Hettner nor I, say that some phenomena are generic, some unique. That is a straw man, put up to be punched down. To be serious for a moment, all of us here, together with the Hottentots and others elsewhere, are equally instances of *homo sapiens*, and for many purposes, therefore, may be studied merely on that generic basis. But all of us, I'm sure, will insist that in many important respects each of us must be recognized as unique. Which, if any, of us is of sufficient significance to justify study as a unique case is a question I leave you to judge.

Hettner stated as early as 1905 that both methods of study were involved in geography, and that successful development of the subject required increased use of the generalizing method, and the search for laws.

This was not peculiar to geography; rather he asserted in 1905 that all sciences -- later modified to "all the concrete sciences" -- are both nomothetic and idiographic. More specifically he pointed to the similar situation in history and in astronomy -- the latter so often mentioned as a prime example of nomothetic science. Windelband had recognized that the study of the evolution of a single species -- of the horse or of man -- was the study of a unique case; and I had pointed to similar examples in political science and in human physiology. In short, exceptional in this important issue is to be purely nomothetic -- true only of the completely abstract sciences.

Every statement I can find in Hettner on this subject, and every statement on it in my writing, declares or supports the view that geography uses both the idiographic and the nomothetic approach. It is not I who misrepresented Hettner's position to American and English readers, but rather those writers who, ignoring my correction of the errors in the Schaefer paper, continue to teach American and English students the direct fallacy that Hettner and I regarded geography as idiographic -- or "essentially idiographic" -- or as not concerned either to establish or to use scientific laws.

I come at last to the concept that you might think logically should have come first, namely, Hettner's views of the place of geography in the system of the sciences -- *Wissenschaften*. His writings in the decade 1895-1905 indicate, I believe, that this concept developed relatively late in his thinking. In 1895 he wrote simply that while many sciences were distinguished by the kind of object studied, geography was one of those distinguished by its method of observation -- *Methode der Betrachtung* -- precisely the expression used by Hermann Wagner in one of his reviews a decade earlier. But Hettner later rethought the question and did not use that phrase in his definitive essay of 1905, or thereafter.

As noted earlier, Hettner's views on the topics discussed

previously were not consequences of his concept of the place of geography among the sciences. The reverse, however, may well have been the case -- that these earlier conclusions suggested the answer to the question of the place of geography. Intriguing as that hypothesis is to me, it is not essential to the question we have been considering. Two aspects, however, need to be noted.

Hettner's statement of this concept was not a simple repetition of Kant's analogy of geography and history. Rather, he spoke of two groups: on the one hand, geography and astronomy, the sciences of space (*Raum*); and on the other all the historical sciences (*Wissenschaften*), including specifically literate human history, pre-history of man, life history or paleontology, and earth history generally -- both groups, of course, in contrast to the systematic sciences each distinguished by kinds of objects studied. Further, he recognized that historical or geographic studies commonly carried out within individual systematic fields constituted an intermediate or overlapping category, having some of the characteristics of each of the related groups.

Finally, to return to Schaefer's concern for geography as a science, Hettner's concept was not a definition of geography; it was not a basis for claiming other similarities among the fields included; it had no controlling authority -- in sum, one might say it has no practical importance; it is nothing to be afraid of.

To conclude: In respect to each of the various aspects Schaefer included under his term "exceptionalism," we found that geography in Hettner's terms was similar in each case to a number of other sciences, a number varying from few to most. I submit, therefore, that "exceptionalism" is a misnomer that in the interest of clarity and integrity of communication should be rejected.

What real issues remain? One or two writers have suggested that if one corrects the factual errors in the Schaefer paper no essential issue remains. That would be unfair to Schaefer's main thesis, and I gather that of his followers. This apparently requires that to be acceptable as a science, geography must be nomothetic, period; whereas I insist, for myself -- and for Hettner, if I may -- that geography must continue to be both nomothetic and idiographic, whatever decree from whatever authority shall be allowed to determine the definition of science.

If this means that geography must recognize that at times in its pursuit of knowledge it is acting as a science, at others not, I suppose we can say so, provided also that astronomers who study the surface of Mars or the chemical peculiarities of Saturn will do likewise, together with the physical anthropologist who studies the evolution of the human species

-- leaving the physicist and chemist alone in the status of pure scientists.

More seriously, why is this necessary? Speaking to our colleagues on the other side of this issue: What have you to fear? For that matter, what did you ever have to fear from the writings of Hettner and me? What studies in the quantitative revolution were in conflict with any of Hettner's teaching, as I have just presented it? Quite honestly, I have never understood what the shouting was about, what freedom you lacked that made necessary the violence of unbridled attack on major works in our heritage, and the continued abuse of scholarship in repeating statements long since shown to be untrue.

In sum, I second Leonard Guelke's motion -- in the August, 1983 issue of *The Professional Geographer* (vol. 35, 3, pp. 336-338) -- calling for a higher level of scholarship in the next thirty years.

Let me then close on a note of encouragement. Someone called my attention to an article by an author unknown to me, named Norman Graves, Professor of Geography and Education of the University of London, in which I came across what seemed a typical example of transmitted error: "Hartshorne's firm statement that the idiographic emphasis in geographical studies must of necessity dominate over the nomothetic emphasis. . . . no worthwhile laws could emanate from the kind of work geographers were doing." Finding the article otherwise thoughtful and carefully written, I thought it might be worthwhile to write the author my conviction that "nothing I had written could justify his interpretation" (R. Hartshorne to N. Graves, April 16, 1982). I received a prompt reply, in which he expressed surprise together with the intention of rereading the sections of my writing I had cited. Some weeks later he wrote me again, enclosing a copy of a letter to the editor of the journal in which he had published the original statement, requesting that they publish a correction, stating: "I can confirm that the above statement is not accurate and that Hartshorne viewed nomothetic and idiographic methods as equally necessary" (*Terra*, vol. 96, 3, 1984). I salute Professor Graves for his courage and integrity. May his tribe increase!

On the Personal Background of Carl Ritter's Geographic Theory

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THE HISTORICAL BACKGROUND of Carl Ritter's geographic theory includes the whole past of geographic thought down to his time. It is not intended here to try to summarize that past, but to direct attention to immediate personal circumstances. Certainly no geographer of the past deserves more searching inquiry than does Ritter. The more one examines his writings -- and a complete examination of them is a long and hard task -- the more one finds in them what is familiar from later geographic literature. Characterizations of the surface of the earth by large regional units; detailed descriptions of smaller areas; essays on the distribution of products having economic importance; historical accounts of the movements of peoples; reconstructions of the occupation of the land in past times; interpretations of history in terms of the physical landscape; all these types of discourse, still found in abundance in geographic literature today after Ritter's long career came to an end are to be read in his writings. One type of familiar geographic discourse is almost entirely lacking. That is inquiry, on the basis of general physical principles, into phenomena of the inorganic constituents of the earth.

Not only are all these species of geographic discourse found in Ritter's writings; some, such as the investigation of the spread and distribution of economic products, are his invention; to others he gave currency, fixing them as constituents of a self-conscious geography. More important yet, by occupying for thirty years the first significant academic chair of geography he set a pattern for later academic activity. If we are to understand the background of our own position in the scientific and academic world, a close examination of Ritter's scientific activity is indispensable. Sometimes it may seem that a scientist arises who is independent of his age; whose work would, indeed, have adorned any time, not being bound to its own period as is the work of ordinary mortals. Such a place is accorded, for example, to Isaac Newton. If these timeless figures are fewer in the history of science than in the history of the arts -- if one can think of a half-dozen Shakespeares and Beethovens to one Newton -- it is undoubtedly because there is a more definite continuity of development in the sciences

than in the arts, because the accomplishments of the timeless genius in science become the common property of later generations of lesser figures, whereas in the arts only the products, not the methods, can be transmitted to posterity.

Carl Ritter was not one of these timeless heroes of human culture. Vast diligence he had; and a power of combination of details that is to be respected. But in the approach to the problems raised by the material with which he chose to work he was the child of his time. We may define that time, in years, by saying that his youth fell in the eighteenth century -- and his maturity and age in the nineteenth century. These dates are important; for a difference of relatively few years made a great difference in the mental set of those of his countrymen whose lifetimes spanned the latter part of the eighteenth and the early part of the nineteenth centuries. Perhaps the great difference in outlook and activity between Ritter and Humboldt may be ascribed in part to the fact that Humboldt was ten years older than Ritter; that as a consequence of the decade of his birth more of Humboldt's formative years fell in the period of intellectual enlightenment in the eighteenth century than in the period of German reaction against extreme intellectualism in which Ritter's youth fell.

These facts, in addition to knowledge of Ritter's personality and personal experience, are requisite to an understanding of what academic geography has been and has become. At a certain point in the orbit of European, more specifically of German, intellectual activity, academic geography was thrown off, and given a direction in accordance with the tangent to the curve of that orbit in Carl Ritter's time. It happens to have been a peculiar time, a time when the orbit of German intellectual life was sharply curved. One may say confidently that if the professorship of geography at Berlin, and with it the example for later academic geography, had been founded fifty, perhaps even thirty, years earlier or later than 1820 the status of geography today would have been different from what it is. Ritter was forty-one years old when he entered on his duties at Berlin. The years of his professorship there were therefore not years of development, but of fulfillment. What he was at Berlin had been determined by his earlier career, very little of which was lived in the academic grove. The most important constituents of his geographic theory were contributed by his earlier intellectual experiences.

* James Parsons discovered this essay in the files of John Leighly after the latter's death, and kindly made it available to the editor of History of Geography Journal. It is reasonable to suppose that the essay was written during the earlier part of Professor Leighly's retirement.

Before proceeding further, the theoretical part of Ritter's contribution to geography should be defined. Of his theoretic views Ritter leaves no doubt; besides the introduction to the *Erdkunde* contained in the first volume of both editions, and the introductory parts of later volumes, we have the series of memoirs published in the *Abhandlungen* of the Berlin Academy. These parts of his writings were collected into a single volume and published in 1852.¹ By so publishing them, Ritter himself separated the more theoretical parts of his writings, specifically for the purpose of "founding a more scientific treatment of geography," from the more objective, factual mass of material that fills the long row of plump volumes of the *Erdkunde*. And indeed there is a gulf between these theoretical writings and the mass of objective information in the *Erdkunde*, which Ritter was able to bridge only by assertion; one cannot say that his theoretical conclusions follow in any sense from the factual data, vast as is the mass of those data assembled in the *Erdkunde*. Both theoretical and objective writings have had their influence; the former by providing a pattern and often the terminology used by later generations of geographers in defining their aims, and the latter by setting an example for later geographic studies. The gulf between the theory and the practice of later geographers has not always been so wide and so deep as in Ritter's work, but it has often been conspicuous. And, it may be remarked, the worse intellectual mistakes made by later geographers have been committed when they attempted to make their objective work conform to theoretical principles rather than to derive theoretical principles from their own and others' objective work.

The essentials of Ritter's method and theory are implied in the title of the *Erdkunde*.² That title reads, in translation, "The lore of the earth in relation to nature and to the history of mankind, or general comparative geography as a firm foundation for the study of and instruction in physical and historical sciences." This long title should certainly not be looked upon as a mere following of fashion. It appears in volume after volume, from the first volume of the first edition, published in 1817, to the nineteenth volume of the second edition, published in 1859. For forty-two years it was the motto under which Ritter presented his work to his contemporaries; and he must have thought it out carefully before it was first set into type for the title page of the first volume. First, the content of the work is intended to place the lore of the earth in relation "to nature and to the history of mankind." We are certainly to understand "nature" here as including the entire visible universe outside ourselves. The earth is a part of this visible universe, and to us by far the most important part. But the part of the earth with which Ritter concerned himself primarily in the *Erdkunde* was the land surface, and particularly the horizontal and vertical distribution of the solid masses of the land. The other constituents of the material making up the en-

tire earth appear as a part of the "nature" in relation to which the specific object of Ritter's attention is to be seen.

The second relation of earth lore is to the history of mankind. This relation is to Ritter, as he says in many places, by far the most important. Humanity, in his eyes -- and this is fundamental to any understanding of Ritter -- was the final and highest constituent of creation, for the sake of which, ultimately, the rest of creation exists. Humanity is bound to the earth; and in the different parts of the earth has developed differently. The essential parts of Ritter's theorizing are connected with this relation; and in expounding it he departed farthest from the sober objectivity with which he handled the immediate material incorporated into the *Erdkunde*. His intention was to include in the whole of his work all the relations implied by his ambitious title. He described *Die Erdkunde* in writing to his brother Johannes after finishing the first volume in 1817, as "a general physical geography, in which are included all the laws and conditions under the influence of which the great multiplicity of objects and tribes and human beings are generated, modified, disseminated, and developed on the earth."³ In the introduction to the book itself Ritter was much more cautious. Here he describes the task to which he addresses himself as being the establishment by inductive study of facts concerning the earth, the details of the great general system of relations that forms the abstract frame of terrestrial phenomena. He did not flatter himself by thinking that he had made more than a small contribution to so gigantic a task.

He emphasized the necessity of an inductive approach to the complicated problem of "localization of facts of nature and their reflection in the life of peoples whose residence and character are coterminous with this or that combination of constituents of the earth." Immediately after this warning, however, he proceeds to a bold assertion: "For under the control of a higher order of things both peoples and individuals arise, under the influence of the activity of nature and of reason, from immaterial and material components of the all-inclusive sphere of universal life."⁴ That generalization could not have been arrived at by the inductive procedure Ritter had just characterized as necessary for the study of the earth.

The main title thus contains the announcement of a theory: that there is a great, inclusive system -- "nowhere does chance rule" -- that includes all phenomena on the earth, inorganic, organic, and intellectual; and that it is the task of geography to investigate this system. The alternative title, "general comparative geography," is more an indication of method. The epithet "general" signifies that the science is to be autonomous, not ancillary to other disciplines, directed toward universally applicable rather than partial conclusions. The term "comparative" characterizes his procedure, the inductive grouping of facts into categories that proceed from the facts themselves. We have, therefore, in the title itself and the explanatory

introduction to the work, a distinction between theory and method, a distinction that may be followed through all of Ritter's writings. This is the distinction referred to earlier between general conclusions concerning the relations of humanity to the part of nature constituting the earth outside the individual, and the laborious compilation and systematization of facts that made up the great bulk of the *Erdkunde*. I shall use the terms "theory" and "method" to characterize these two aspects of Ritter's work. Both theory and method will be discussed in the following, since they are inextricably interwoven in the fabric of Ritter's thought. The "method" was an immediate consequence of his conception of geography as a "historical" or "empirical" discipline, the data of which have been accumulated by observation in all parts of the earth, are recorded in the writings of travelers, and must be put together inductively. We may agree, I think, that the inductive investigation of terrestrial phenomena may be carried on without any presumption of an absolutely inclusive and systematic frame of relation among all the phenomena Ritter included in his view. It is his insistence on the existence of such a frame, not as a hypothesis to be tested but as an axiom, that Ritter's mental operations differed from ours. It is in this respect that he appears most distinctly as an individual, but as an individual belonging to his age. In order to understand his bias, even in part, it is necessary to examine his experiences previous to the writing of the *Erdkunde*, and the intellectual climate in which he grew up.

To one who follows Ritter's life through the account of it given by his brother-in-law G. Kramer, the line of his development is clear and continuous from his early childhood to the time of his entrance on his duties at Berlin in 1820. At the age of six he and his older brother Johannes were placed in the newly established school of Christian Gotthilf Salzmann (1744-1811) at Schnepfenthal in Thüringen. He remained there as a pupil for eleven years, until he went to Halle for his short career as a university student. His father had died when Ritter was only two years old. When the Ritter brothers went to Salzmann's school, they were accompanied by a young man, J.C.F. Gutsmuths (1759-1839), who had been a tutor in the Ritter family and who remained at Schnepfenthal as a teacher. The fatherly care and instruction given Ritter at Schnepfenthal by Gutsmuths and Salzmann shaped his mind; their influence can be traced throughout his career. After leaving Schnepfenthal he returned to this scene of his childhood as often as opportunity permitted, so that the influences of the methods of teaching practiced there remained active long after his schooling was ended.

Salzmann's school was one of the many institutions that grew out of the urge toward a reform of pedagogy released by the humanitarianism of the Enlightenment.⁵ In reaction against the formalities of existing schools, and to a marked de-

gree influenced by Rousseau's ideas concerning education, the progressive teachers of the late eighteenth century, Salzmann among them, turned their attention away from books and the rigid discipline of the schoolroom. At Schnepfenthal the pupils had plenty of bodily exercise in the form of useful work, sports, and long walks in all kinds of weather. Much emphasis was laid on becoming familiar with nature at first hand. Entirely in accordance with the remainder of the program was the religious instruction imparted, the intention of which was "to lead the children to a knowledge of God through the contemplation of nature."⁶ It was eighteenth-century deism, in a form that approached pantheism, that the pupils in Salzmann's school learned. The same naively conceived relation of God and nature runs through all of Ritter's theoretical writings. Ritter's conception of God in nature went through some modifications in later years, under the influence of the idealistic philosophy that was current in the years of his maturity, so that God lost some of the features of a larger "Father Salzmann" and approached the more abstract spirit of nature of the romanticists. But there remained a good deal of the childish conception of God as the fairly immediate Creator of the universe, the Author of the natural laws the universe exemplifies, the Being behind nature whose ways the diligent and humble scientist may learn through study of His works. In this persistent attitude we may recognize the lasting influence of the religious teaching Ritter received from Salzmann in his childhood.

Considering the manner in which his teachers Gutsmuths and Salzmann served in Ritter's childhood and early youth as substitutes for a father, it is not astonishing that he decided, as he approached the time when he would have to leave Schnepfenthal, that he wanted to become a teacher. In order to realize this ambition further study at a university was necessary. It was at this critical time, in 1795, that relations between Ritter and the Bethmann-Hollweg family in Frankfurt, which were to last for nearly twenty years, were first established. The Hollwegs, to make a rather long story short, promised him a position as tutor for their children after he had attended the university. Bethmann-Hollweg recommended in particular that Ritter attend the university at Halle, and study under Professor A.H. Niemeyer, who enjoyed a wide reputation as an authority on pedagogy.⁷ First, however, Ritter remained at Schnepfenthal for an additional year, partly in order to learn something about teaching from Salzmann, partly to gain further preparation for the university. One gathers from the correspondence reproduced in Kramer's biography of Ritter that his preparation for the university was not of the best. Salzmann himself, Gutsmuths wrote to Ritter's mother, was of the opinion that Ritter lacked the mental capacity for university work.⁸ Some of the defects of his schooling were patched up by this extra year at Schnepfenthal, and he entered the university at Halle in the autumn of 1796.

Since his two years in Halle were the only ones Ritter spent in formal academic study, it is of interest to note the subjects he pursued during that time. Kramer gives full information on only the first three semesters. In these Ritter had the following work: Two semesters each of European political history and geography, finance and police administration, philosophy (including logic, metaphysics, esthetics, and ethics), and physics. One semester each of chemistry, architecture, and Roman history. Of his work in the fourth semester of residence, Kramer mentions only pedagogy, in which subject he attended lectures by Niemeyer and read widely both in the university library and in Niemeyer's private library.

With these studies, at the age of twenty, Ritter's formal instruction came to an end. The preparation he had acquired was undoubtedly adequate for the position of tutor into which he was immediately to step, though as the Bethmann-Hollweg boys grew up he discovered serious gaps in it. His rather informal schooling at Schnepfenthal, and two years of diversified study at Halle was not a good preparation for a scientific career. Little of his later accomplishments can be referred back to any of this instruction.

So began his long career as tutor in the Bethmann-Hollweg family, which was to last until the younger of the sons, August, who was three years old when Ritter went to Frankfurt in 1798, entered the University of Göttingen in 1813. In these years Ritter guided the two boys through their studies, faithfully applying, as nearly as conditions would permit, the pedagogic principles he had learned from Salzmann at Schnepfenthal, and visiting his friends there as often as he could. Of necessity he had to give close attention to the subjects his pupils were studying at any particular time, so that this attention was distributed over the whole range of material the boys had to cover. These were Ritter's formative years, the years of his young manhood. His associates were mainly teachers: either private tutors in other families or teachers in the Frankfurt schools. The general ideas with which he concerned himself were mainly pedagogic. The world, it would seem, shaped itself according to pedagogic theory. His aim in his teaching was to produce fully rounded human beings. Education and life were to be unified wholes. So were the subjects taught. The world with which his pupils were to become acquainted was to be comprehended as a whole. His teaching was not the treadmill grind of the classroom teacher, whose cycle of activity is closed in one year. There was here only one cycle, which led two boys from their early school years to the university, and which was not repeated. The intense sense of unity of nature that Ritter displays in his later writings is the unity he sought to give to the cycle of studies through which he led his pupils during these decisive years.

In these years, too, Ritter began his activity as an author. The writing he did during his period as tutor -- his biographer

lists eleven titles published during this period -- follow through the cycle. They began, while his pupils were still at a tender age, with an article on the teaching of drawing (1802). Then follow some pedagogic fragments, which display no definite tendency. No special interest in geography is reflected in a passage in a letter written in 1802: "This week, thank God, I have finished the second very complete and last course in the geography of Europe, and now, to my great joy, I am starting the boys on history."⁹ His writings on geography began with the first part of a small work on Europe (1804), which was followed by a second volume published in 1807. Like so many of his later writings, this work was never completed. Between these dates appeared "Six maps of Europe with explanatory text" (1804-1806). An article entitled "Some comments on the method of instruction in geography" (1806)¹⁰ falls in the same period. One does not get the impression that Ritter had as yet become particularly interested in geography, in spite of these early writings. At one time he assured his employer, Frau Bethmann-Hollweg, that the writings he was publishing were not done independently of his teaching, but were the products of his working up material for use in teaching her sons. And at another time he expressed to her (he was in the habit of making written reports to her at intervals) his satisfaction that he was about to begin teaching the boys physics, "my favorite science." If his preparation of teaching material in geography yielded more publications than did his preparation of other subjects, it must have been because he found geography in a poorer state of organization than these other subjects, so that what he prepared was newer and of more interest to others and so worth publishing. His objections to the state in which he found geography were that it did not provide "the large view of the world," the view that required the presentation of "detail not as detail, but in relation to the whole, as a preparation for a view of the universe." This is a pedagogic objection; a reform of geography was needed because the existing state of the subject did not make it a part of the unity of nature.

I do not think it is an exaggeration to say that Ritter was led to concern himself with the organization of geography because it was precisely the subject that required the most work to bring it into the frame of his pedagogic ideas. Unity of the individual, unity of education, unity of the exterior world; these were watchwords of Ritter's pedagogy and of the best pedagogy of his time; and geography needed to be molded into that unity. In the foreword to the first volume of his textbook on Europe (1804) he expressed his intention to present "a living view of the entire country. . . as a coherent whole."

I attempted to bring everything, so far as was possible, into relation, and to present it as cause and effect. . . The earth and its inhabitants have the most precise interrelations with each other, and the one can not be presented in all its relations without the other. Therefore history and geography must always remain in-

separable companions. The land affects the inhabitants and the inhabitants the land.

Again, Ritter laid primary emphasis on the unity of the constituents of the earth; incorporated into this unity is the ancient thesis of the influence of the earth on its inhabitants. But there is also the assertion of a reciprocal reaction between land and people, implying an effect of the people on the land as a part of the great general system of interrelation.

The next impetus to Ritter's activity as a writer on geography came as a result of a visit to Johann Heinrich Pestalozzi's school at Yverdon in Switzerland in 1807. It was inevitable that Ritter, with his practical and theoretical interest in pedagogy, should be attracted to this institution; and in his account of his visit, published in two pedagogic journals (edited respectively by his former teacher GutsMuths and his stepfather Zerrenner) he spoke with the highest enthusiasm of the work of Pestalozzi and his collaborators. The most important consequence of his visit to Yverdon was a request by Pestalozzi that he write a handbook of geography in accordance with Pestalozzi's general method of instruction. This was the origin of Ritter's handbook of physical geography which was never published, but the plan of which reappeared later, in unified form, in the *Erdkunde*. The part that Pestalozzi's method played in the genesis of this work appears from a long letter from Ritter to his stepfather Zerrenner, written in May, 1810. He wrote that he began the promised work,

but found that the materials I had to work with were fragmentary and unconnected, that the science had been handled capriciously. But when I then in the spirit of the Method (for the formulators of the Method know nothing themselves of geography) shunned all that was fortuitous and sought out only what was related by necessity, I found my way, I believe, happily out of geographical chaos. Now that I had found the thread, the whole tangled knot unraveled itself; and I found in my geography, which beyond its satisfaction of the understanding exalts the heart through the high wisdom and regularity that it everywhere reveals, a not unimportant contribution to natural theology.¹¹

In this letter Ritter used expressions that recur in his later writings regarding the insight gained by his investigations, especially into the early movements of peoples. Of more importance is the evidence of a profound interest in the subject; the tone of the letter is very far from the "Thank God" of a few years earlier.

This work of Ritter's was never published, but its contents and organization are presented in a good deal of detail in a letter incorporated by Kramer into his biography of Ritter.¹² Its title, in translation, reads thus: "Handbook of general earth lore (*Erdkunde*); or, The earth, a contribution to the establishment of geography as a science." The earth was dealt with

as earth; its relations to other heavenly bodies, thus the whole of mathematical geography, was omitted. The distinctions among the several parts of the earth that were recognized were the distinctions provided by the earth itself, not by political geography. The order of presentation was from the most general to that which is more dependent, and so down through classes to individuals, "in everything that nature displays." The procedure was from the simple to the complex, "in areal, temporal, and physical relations." By such a procedure the rule or law governing the relations emerges. In reference to this arrangement Ritter characterized his procedure as in accordance with Pestalozzi's Method. The work was definitely intended as a guide for teaching, despite the proud aim declared in its title.

The "Handbook" was divided into three parts, which Ritter designated respectively "topical, formal, and material," and explained further as comprising, in order, "place knowledge, form knowledge (physical geography), and knowledge of the components (geography of natural history). The topical part is also the first course in geography. It forms a preparation, and sets the pattern for the procedure from the natural division of the earth's surface." "The formal part presents the principal forms and constituents of the sea, the atmosphere, and the land, in their reciprocal relations. It gives. . . the basis of all geography. In these interactions lie all the external impulses on human beings and peoples that have determined the main directions of their life in history." "The third, material part contains the general and particular laws of the distribution of the objects of the three realms of nature over the entire earth, together with characteristic forms, the capacity for dissemination, and the history of the migrations of minerals, plants, and animals. It contains in addition the lore of climate, which is the condition of the distribution of organisms, and the parts of geology that assist in the understanding of the distribution of inorganic objects."

Here is an arrangement of material that is logically appealing. First, the place knowledge that must be had if information about the earth is to be organized into any systematic form. Second, the physical pattern into which place knowledge is to be fitted, proceeding from the fluid masses of the earth to the land. Third, individual products of nature in their distribution. The discussion of distribution can build on the knowledge of the system of distribution of the fluid and solid components of the earth arrived at in the second part, and on the place knowledge gained in the first. The third part of the projected book deserves special treatment. It points forward to the monographs on products that Ritter wrote later in his life, and to the essay on a science of products,¹³ which to the modern scientist are probably the most interesting of his writings. Ritter reported to his stepfather that in writing the book he had "advanced a few paces further" than his

predecessors, especially in regard to "ocean currents, winds, the distribution of mountains and plains, the formation of river valleys, physical climates, and the distribution of minerals, particularly deposits of salt and coal." He mentioned further that he had arrived at "a history of the peopling of the earth by plants, animals, and mankind, which reaches back into the past beyond history." Thus, he continued,

I have learned the great migrations of marine animals, fishes, land animals, and their limitations to certain districts by the progress of culture; the dissemination of tropical plants by currents, of the grains by the migrations of peoples, and of fruits by civilized peoples. Thus I was led to the original homes of peoples, and followed thence the migrations and dissemination of the human race over the whole earth. . . . So I believe that I have, in this system of physical geography, documented the basis of a scientific geography in general and all the exterior stimuli to the development of peoples.¹⁴

Familiarity with the discrepancies between the utterances Ritter made in his personal letters and the actual content of his published writings makes one cautious about taking this fairly large boast literally. It can not be doubted, however, that in the work written under the influence of Pestalozzi he had accomplished a very creditable organization of the elementary material of geography. It is greatly to be regretted that the book never saw the light; for it was clearly of such a character as to exert a strong and healthy influence on the development of geography if it had been published. It is to be noted that the new insights Ritter reported as having gained fall in both the second and third parts of the work; insights into the system of arrangement of the inorganic constituents of the superficial parts of the earth and into the dissemination of organisms. This "handbook of general earth lore" was the immediate ancestor of the *Erdkunde*. That Ritter looked upon it as such appears from a remark contained in a letter to his brother Johannes written in 1815, when he was working on the first volume of the *Erdkunde*. He remarked on this occasion that this task "had busied him for eight years."¹⁵ Eight years go back exactly to 1807, the year in which Pestalozzi asked him to work up a manual of geography in accordance with the Method. We need not hesitate, therefore, to think of the *Erdkunde*, in Ritter's first conception, as a revision of the "handbook" of 1810, the publication of which Ritter (who seems to have been highly sensitive to adverse criticism) abandoned because Leopold von Buch found fault with his discussion of mountains.¹⁶

Let us look now at the arrangement of the material of the "handbook." The content is mainly the traditional material of instruction in geography. The rigorous grouping and order of presentation are the important things. Yet even this order has something familiar about it, to one who has read earlier in-

clusive geographic treatises. What it recalls is not Immanuel Kant's lectures in physical geography, a version of which had been published in 1802. The strict and rather dry logical organization of Kant's work could not appeal strongly to Ritter, who in his student days had found logic "*ein saueres Stückchen Arbeit*," since he "lacked too much the ability to hold [his] thoughts together and to direct them toward a single object."¹⁷ The earlier work that is recalled is Torbern Bergman's treatise,¹⁸ which was familiar to all Germans who worked with geography in the later eighteenth century. Bergman proceeded from a general discussion of the dimensions of the continents to a sketch of the continents and the islands of the earth. This section corresponds to Ritter's "topical" division, "place knowledge." In Bergman it is brief, and would form no more than an outline of a first course in geography; Ritter obviously expanded it. Bergman then presents four chapters on the relief and structure of the land. This material Ritter distributed among his second and third divisions. Bergman's next sections, on water and the atmosphere, correspond to the order of discussion of these two subjects in Ritter's book. Bergman's section on organisms corresponds to Ritter's third division, though Ritter puts mineral products under the same head as these. The strongly dynamic element in Ritter's treatment of "products," and his confidence in the existence of a system comprising the whole, are prominent in Bergman's treatment of the earth:

everything is in movement. . . . formation and dissolution alternate over the entire field of view. But these changes do not proceed, as may appear at first, without order and limits. Everything proceeds according to certain laws, everything is ordered toward certain ends. . . . The whole interconnection is as yet unknown to us, but from what has already been discovered we can not doubt its existence.

Even the final assumption of a purpose in the universe, directed toward humanity, was made in scientific circles in Uppsala as well as in Salzmann's Schnepfenthal: "So far as we know there is no other animal on earth besides man that through contemplation of nature can be led to knowledge of the Creator. For his sake, then, this planet has been prepared."¹⁹ It is easy to trace Bergman's arrangement of material back to Varenus. Ritter takes his appropriate place in the main line of development that geography has followed since the Middle Ages. His strong pedagogic interest, articulated with the best pedagogic thought of the time -- and that best thought was of a very high quality -- brought the traditional material into a more disciplined organization than it had previously been given.

A stricter organization of material and the incorporation into the organization of inclusive general concepts under which the minor categories of the material can be grouped have more than a mere pedagogic value. They serve as well as

a guide for research, by establishing an improved frame of reference into which the results of research may be fitted. The organization identifies minor divisions of an entire field that may in turn function as units of investigation. Organization for pedagogic purposes may not be the same as organization according to strict logical principles, but in the development of a field of knowledge it may serve the same purpose.

It is thus, I think, that Ritter's work on the "handbook" of 1810 affected his later work. The pedagogic order of the material in the "handbook" became the chronologic order in which he resumed the task of organizing geographic knowledge a few years later when he began the *Erdkunde*. This was sometime between 1813 and 1815, when his residence at Göttingen in the company of August Bethmann-Hollweg gave him an opportunity to use the university library.

By the time Ritter began the first edition of the *Erdkunde*, his organization of material had been somewhat modified. The plan of the whole work is outlined in the introduction to the first volume. The tripartite structure is carried over from the "handbook" of 1810, but in the new work, which did not have an immediately pedagogic purpose, the "first course," place knowledge, was not needed. Instead, Ritter brought up, for the "first part" of the *Erdkunde*, the material on the land from the second section of the "handbook," and gave it a separate and the most prominent place in the new work. The *Erdkunde* that Ritter had in mind when he began work on the first volume was to consist of three parts. The two volumes published before Ritter gave up the series and began on the second edition constitutes the beginning of the first part, to be devoted to the land. The second part was to deal with "the fluid forms: the water of the oceans, of the seas, of streams, and of the atmosphere" -- or, as we should say, the hydrosphere -- and in addition the atmosphere, in general, and "in particular that part of it that is modified by contact with the surface of the land and the sea, the seat of activity of climate." In contrast to the "fixed," stationary land, the fluid forms are the mobile, circulating components of the surface parts of the earth. The third part of the *Erdkunde* as Ritter planned it was entitled (in advance) "the bodies of the three realms of nature;" it is identical with the third division of the "handbook." These three parts would form the "general comparative geography." In order that the view might be complete, Ritter thought it would be necessary to have a supplementary work on mankind, which would investigate the inner urges of humanity, the purely intellectual components in the interaction between nature and man. This work he left to others.²⁰ In his revised organization of earth knowledge Ritter returned, interestingly enough, very nearly to the organization of Bergman's work. It was the final organization Ritter arrived at, and was followed, at least formally, through the two volumes of the first edition of the *Erdkunde* and into the earlier volumes of the

second edition.

But once the work, the bulk of which Ritter evidently did not foresee, was begun, what was initially planned as the first part of a whole gradually became an end in itself. In the introduction to the first volume Ritter announced that the work was undertaken "for the sake of the history of mankind and of peoples;" the understanding of humanity rather than of nature was his ultimate aim. Hence, in the systematic descriptions of the regions into which he divided the continents, discourse concerning the peoples who inhabit these regions was introduced. As the work proceeded, ethnographic and economic information was added to the chapters devoted to descriptions of the surface of the land. Thus, these chapters gradually took on the form of regional monographs. The transformation was accomplished only in part in the first volume of the second edition of the *Erdkunde*, devoted to Africa, which appeared in 1822. It is complete in the second volume, the first volume on Asia, which appeared, after a ten-year interruption, in 1832.²¹

What brought about the transformation? Ritter's biographer Kramer does not reproduce enough of Ritter's correspondence from his Berlin period to permit one to trace his intellectual development as clearly as one can through his earlier years. It would be strange, however, if the routine activities of his life at Berlin did not affect his scholarly work as it did in his youth. His removal to Berlin in 1820 brought him into a new round of duties and a new circle of associates. The first volume of the second edition of the *Erdkunde* was written early in his career in Berlin, in the leisure he enjoyed because no students appeared at his announced lectures at the University. Though more material is incorporated into it than into the part of the first volume of the first edition devoted to the same continent (Africa), it displays little advance in method. The ten years that intervened between its appearance and that of the next volume were busy ones; years in which the habits that persisted through the remainder of Ritter's life were formed. The work to which he gave most time and attention was not his lecturing at the university, but the more detailed and intensive formal teaching at the state military school. The subject he was engaged to teach at that institution was *militärische Statistik*, presumably a sort of politico-military geography. When the invitation was first made, the expression meant nothing to Ritter: "I don't know exactly what it means," he wrote to his brother.²² What he made of it was a regional geography of Europe, worked out according to the method he had followed in writing of other parts of the earth in the *Erdkunde*. The nature of the instruction he gave appears from an outline he submitted to his superiors in 1827, which Kramer reproduces in his biography.²³ In this outline the continent is divided according to relief and drainage basins, in the detail that a year's course of four classes per week permitted. As in the first volume of the *Erdkunde*, the regional discussion is

preceded by a general review of the earth and a definition of terms to be used. Cultural and economic information occupies a small place. A further development was necessary before his plan demanded the inclusion of climatic, geological, botanical, and zoological constituents, as it did in the first volume on Asia, published in 1832.²⁴ In the light of Ritter's earlier development, are we not justified in concluding that the transformation of the *Erdkunde* was the consequence of his routine work at the military school and the university? His classes at the military school required a rigorous organization of detailed information concerning a relatively small continent, Europe. His lectures at the university, always more general in tone and freer in their composition, provided an opportunity for formulating general views. The years he spent as tutor led to a general earth-lore, the divisions of which had an order of their own, deduced from general pedagogic principles. His years as instructor, primarily in the geography of Europe, led to a lore of regions, in which the concept of unity, formerly applied to the whole of nature, was narrowed down to small divisions of the earth's surface. The sequence of relief, hydrography, climate, and products, which in the introduction to the first volume of the *Erdkunde* was applied to the earth as a whole, was now brought down to the region.

The chain of development of Ritter's *Länderkunde* can thus be fairly easily traced. His own formulation of a definition of it comes late in his career, probably most distinctly as late as 1854: "The specifically geographic field is the lore of regions occupied by terrestrial objects (*irdisch erfüllte Räume*) and their influence on the course of development of the history of peoples."²⁵ This definition contains two parts, which to the modern mind seem to fit together in scientific discourse only imperfectly. On the one hand the components of an area, on the other the influences exerted by the totality of these components on the history of peoples inhabiting the area. How can such disparate elements be combined into one unified discipline? Only by viewing them, not as disparate elements, but as parts of one whole inseparably bound together. The understanding aimed at in the definition could be gained only if the investigator used the methods of all the sciences, which would be needed if all the interrelations of the complex object of study were to be elucidated. Enough difficulty would be encountered if only the constituents other than humanity were to be investigated; but the difficulty, seen from our point of view, is multiplied when humanity is included in the complex. The procedure Ritter announced in the first volume of the *Erdkunde* is, briefly, as follows:

His earth lore did not undertake to investigate the components of the earth's surface as masses or as matter, "for this is the task of other sciences. Our task is to consider the configurations that they assume, in their mutual relations, with respect to the terrestrial globe,

and the consequences of these configurations." The consequences of the spherical form of the earth, if we were to investigate them in detail, "would lead us into the field of mechanics, physics, chemistry, physiology, and other sciences, the truths of which we here assume as given." It is not the history of the changes and transformations brought about by the interactions among the constituents of the earth that are to be investigated, for that is "the task of a physics and an archeology of the earth; rather, our principal object of attention is the result [i.e., of those interactions] in moments of equilibrium, or adjustments and approaches to equilibrium. We seek the present relations of the configurations found on the earth's surface, and in the changes what is determined by the processes now at work."²⁶

Exclusion of investigations into the mechanics of change, if rigorously adhered to, would seem to exclude the possibility of recognizing dynamic interrelations among the diverse objects under examination. If the relations that may be recognized through the application of principles derived from mechanics, chemistry, and so forth are excluded from the discussion, it is difficult to see what principles are to serve as a guide to the selection and arrangement of material, and in terms of which the relations sought for are to be expressed. If we examine the *Erdkunde*, we find that Ritter did not adhere strictly to the principles he laid down at the beginning of his work. Particularly in the second edition, he did not hesitate to apply the principles that relate, for example, temperature of the air to distance from the sea. Where he was unable to establish such relations, the facts covering the several categories of phenomena on the earth's surface are set down systematically, but the relations among the categories are not established. What, then, was the organizing principle that, if not at first, then finally, should inform the structure of the discourse and give it the coherence that Ritter did not find in earlier geographic literature? Ritter admitted, in the introduction to the first volume of the *Erdkunde*, the necessity for "a point of reference, an ideal background." His presentation of that ideal is worth reproducing: "The ideal background, from which the unprejudiced view of facts to be used in their arrangement in this particular manner seems to the author to have proceeded, does not lie for him in the truth of a concept, but in the total content of all truths; thus in the realm of faith. It rests on an inner intuition that has been formed by his life in nature and in the world of men. It was awakened in him in conversation with a great man of the century,²⁷ and in one aspect, as a fundamental idea in this science, was expressed thus: if the work were rightly done, the fundamental ideas must be reflected from all its parts, and be awakened in other kindred minds. Therefore it cannot be defined before hand in its essence, but, playing through the whole work, can assume its complete form only when the work is concluded."²⁸

The organizing principle was thus a matter of faith, inclusive and incapable of being defined in advance. It was derived from Pestalozzi, a singularly exalted character who had found his way through severe inner trials from the rather vague humanitarianism of the Enlightenment to a positive, fruitful basis for his labors on behalf of a better humanity, which was to be attained through education.²⁹ But, just as Ritter eventually absorbed into the part of the *Erdkunde* that was completed, many of the components that originally were postponed to the projected later sections, so, as time went on, he was constrained to anticipate the general conclusions that otherwise were to be reflected from all parts of the work as it progressed and be awakened in kindred minds, or be stated explicitly only when the work was finished. These anticipations were incorporated into the "memoirs toward a more scientific treatment of geography," presented to the Berlin Academy between 1826 and 1849. And as the *Erdkunde* became more and more an end in itself, the general principle belonging to natural theology rather than to empirical science was occasionally brought in. The application of this principle formed the "teleology" that Ritter's critics have never ceased to view as a deadly fault in his writings. The following summary of the "teleology" is translated from a paraphrase of numerous passages in Ritter's writings, written by the later critic who has analyzed those writings most profoundly:

The earth is not only a stage for all the operations of natural forces and natural laws; it is also the stage for all human activity, of history; a stage for divine revelation. The earth stands in a higher relation than does the rest of the visible universe, namely to the invisible universe, to the spiritual nature of all existence, or to the Creator and the rational creatures on the earth; not only to the realm of nature, but to the realm of spirit. In this respect it is God's creation, in the highest degree purposeful, full of beauty and excellence, a divine world, a revelation of divine wisdom in visible form. The earth is not only the platform, the cradle, the dwelling place, but also the school, the great educational institution of the human race. All history of humanity leads definitely to this conclusion. The earth is distinguished from all other objects by this ethical, moral purpose. The earth must have been prepared in its creation and evolution for such a higher end, must thus have been given a higher organization than other objects. The higher purpose of the earth undoubtedly requires a higher organization for its realization, because the physical basis of such a purpose could not be left to mere chance, to a mere material operation of natural forces. The earth cannot be other than the work of divine Providence. The several parts of the earth have also their several peculiar capacities for development as parts of the totality of the world. Every new period of time reveals this more and more clearly. The earth is no finished mechanical clockwork, it is a progressive

divine organism and has a divine mission.³⁰

So much for the object of investigation to which the science of geography is addressed. Geography must have a mission no less exalted than its object. Here is the same author's paraphrase of Ritter's interpretation of the task of geography:

The higher task of the science and its highest goal consist therefore in the cognition (from the regional viewpoint) and the arousing in kindred minds of the intuition of the higher organization and the ethical purpose of the earth; of the earth as the school of the human race. Only thereby does it attain its true coordination, its true unity. That is the ideal background that gives unity to the multiplicity of empirical facts. Only thereby does the science become truly independent and attain a dignity equal to that of the other sciences; its living principle, too, lies in God; it, too, is the creature's hymn of praise to its Creator.³¹

Wisotzki, from whose work the foregoing passages are translated, cites in detail resemblances between Ritter's work and that of his contemporaries in several scholarly fields. They, too, sought connections, lines of development, unity. They, too, piled up mountains of factual data from which the truth might be expected to emerge. On the philosophic side Wisotzki thought he could find in Ritter's highest principle an echo of Schelling's philosophy. The coincidence of method and guiding principles among contemporaries does not necessarily establish direct or conscious mutual influence. There is a component of parallelism common to the paths of intellectual development of all contemporaries. I find little evidence that Ritter paid much attention to formal philosophy. If he had, it seems to me, he would have formulated the theoretical basis of his work more sharply. The intensely religious element in Ritter's nature was there from his childhood; and if it expanded from Salzmann's rather cool deism into a warmer, more emotional form in his Berlin years, and came to be expressed more frequently and emphatically, Ritter was only moving with his time. On his removal to Berlin in 1820 he was introduced by his former pupil August Bethmann-Hollweg to a group the members of which were warmly interested in a revival of religious life.³² In an age when free expression of emotion dominated literary and religious activity, Ritter's sense of the divine in nature and in human history was, understandably, expressed more overtly and forcibly than in the soberer intellectual atmosphere of his youth.

One essential metaphor that recurs repeatedly betrays the continuity of the elder Ritter with the Ritter of the first two decades of the century. The is the frequent reference to the earth as the "nursery," "school," "*Erziehungshaus*," "*Erziehungsanstalt*" of humanity. I call it essential because it was by means of this metaphor that Ritter emphasized the divine

mission of the earth. Humanity has an eternal destiny, and its life on this planet is the education for that destiny. Hence God's careful preparation of the place where this education is to be carried out. Surely it is the boy who grew up in a carefully organized educational institution founded and conducted by a wise and fatherly man who uses this metaphor. Surely it is the youth who, charged in turn with the education of two boys, tried to persuade the parents of those boys to let him take them away from the unfavorable surroundings in the home of their wealthy parents and bring them up in a place specially prepared for the purpose. Surely it is the young man whose career was determined, more than by anything else, by his visits to Pestalozzi's institute in Yverdon. Ritter would have rejected such a suggestion as sacrilege; but with all due respect to his memory, it can be asserted that his ripest view of the earth was that of a vast private school presided over by a magnified Salzmann or Pestalozzi. The ultimate knowledge of the earth to be gained by the geographer was knowledge of the principles on which the school is organized: knowledge of the Method of the Divine Pedagogue.

This essential theory of Ritter's, as theory, is of little importance to us today. With reference to the positive accomplishment recorded in Ritter's *Erdkunde* and in some of the memoirs published separately from it, it is of importance as the inner source of the energy that enabled him to accomplish his vast work. As the *Erdkunde* gradually became an end in itself, Ritter worked out within it those parts we find most constructive today: particularly the essays on "products" -- that is, on useful plants and animals -- several of which are buried in the forbidding array of thick volumes, of the work, but two of which, on sugar cane³³ and on cotton,³⁴ were published outside it. The material for these essays was assembled in the course of Ritter's wide reading for the sake of the *Erdkunde*. They were crystallized out of that material, not under the influence of the ideas that were guiding Ritter in his conscious composition of the *Erdkunde* itself, but as partial fulfillment of the plan for the third division of the projected "handbook" of 1810, which was to deal with the migrations of plants, animals, and minerals. The plan of this division as the prospective third part of the *Erdkunde* as Ritter conceived it in 1817, where its title was formulated as "the bodies of the three realms of nature," remained unchanged. This part of Ritter's writing has not, it seems to me, been given the attention it deserves. Later writers have damned his teleology and praised his regionalism, but have generally ignored his *Produktenkunde*. Yet it is the part of his writings that can be read at present with the most profit and enjoyment.

"The course of development of any science is not determined alone by philosophic discussion and methodologic inquiry; it is rather personalities that exert the decisive influence. The power of individuality usually outweighs all other factors."

So wrote Siegmund G. Unther, the indefatigable student of the history of the natural sciences, looking back over the history of geography in the nineteenth century.³⁵ Few sciences illustrate that generalization better than ours. We still walk in Ritter's shadow.

Endnotes

1. Carl Ritter, *Einleitung zur Allgemeinen vergleichenden geographie, und abhandlungen zur begründung einer mehr wissenschaftlichen behandlung der erdkunde*, von Carl Ritter. Berlin, 1852.
2. cf. Richthofen's definition of 1883 and *China*.
3. G. Kramer, *Carl Ritter. Ein Lebensbild nach seiner handschriftlichen Nachlass*, ed. 2, vol. 1 (Halle, 1875), p. 236.
4. *Die Erdkunde*, vol. 1 (1817), p. 4.
5. On Salzmann and his school cf. K.A. Schmid (ed.), *Geschichte der Erziehung vom Anfang an bis auf unsere Zeit*, vol. 4, pt. 2 (Stuttgart, 1898), pp. 350-380.
6. *Ibid.*, p. 367.
7. Kramer, Carl Ritter, *op. cit.*, p. 34.
8. *Ibid.*, p. 31.
9. *Ibid.*, p. 86.
10. "Einige Bemerkungen" über den methodischen Unterricht in der Geographie," *Neue Bibliothek für Pädagogik*, vol. 2, 1806, pp. 198-
11. Kramer, *op. cit.*, p. 139.
12. *Ibid.*, pp. 174-178.
13. Der tellurische Zusammenhang der Natur und Geschichte in den Productionen der drei Naturreiche oder: Ueber eine Productenkunde, *Abh. d. preuss. Akad. d. Wiss.*, 1836, pp. 205-225.
14. Kramer, *op. cit.*, pp. 138-139.
15. *Ibid.*, p. 235.
16. *Ibid.*, p. 179.
17. *Ibid.*, p. 45.
18. *Physisk beskrifning öfver jord-klotet* (Uppsala, 1766); translated into German; ed. 2, in two volumes (Uppsala, 1773); German translation. Ritter cites Bergman's work in *Erdkunde*, vol. 1 (1817), p. 20, (though he misspelled its author's name) as "the first geographic physics."
19. Bergman, *op. cit.*, ed. 2, v. 1, 1773, pp. xi, iv. I translate from the Swedish edition, since I have not had access to the German translation.

20. *Die Erdkunde*, v. 1 (1817), pp. 13-19.
21. The development of Ritter's regional description is traced by Bruno Schulze, *Charakter und Entwicklung der Länderkunde Karl Ritters* (diss., Halle, 1902). Schulze misses, I think, the true line of development. Fixing his attention solely on the regional aspect of Ritter's writings, he took into account only the *Europa* (1804-1807) and the first and second editions of the *Erdkunde*, apparently on the assumption that Ritter's aim from the beginning was the writing of *Länderkunde*, regional geography. He admits that the form of the *Europa* was the same as that of the stereotyped descriptions by states that were common at the time when this work was written. The first edition of the *Erdkunde* appeared to him, therefore, as an improvement over the *Europa*, and the second edition, in the first six or seven volumes of which Ritter attained the peak of his regional writing, a further improvement over the first edition. I do not think that a conception of the type of regional account represented in the earlier volumes of the second edition of the *Erdkunde* had arisen in Ritter's mind when he began work on the first edition.
22. Kramer, *op. cit.*, p. 293.
23. *Ibid.*, pp. 449-454.
24. *Die Erdkunde*, ed. 2, vol. 2 (1932), p. xvi.
25. *Die Erdkunde*, vol. 17 (1854), p. ix.
26. *Ibid.*, v. 1 (1817), pp. 59-60..
27. Kramer (*op. cit.*, p. 413) identified this man as Pestalozzi.
28. *Die Erdkunde*, v. 1 (1817), pp. 22-23.
29. cf. Arthur Stein, "Pestalozzi und die Kantische Philosophie" (*Heidelberger Abhandlungen zur Philosophie und ihrer Geschichte*, no. 12, Tübingen, 1927).
30. Emil Wisotzki, *Zeitströmungen in der Geographie* (Leipzig, 1897), pp. 300-301.
31. *Ibid.*, p. 303.
32. Kramer, *op. cit.*, p. 339.
33. *Über die geographische Ausbreitung des Zuckerrohrs (Saccharum officinarum) in der Alten Welt vor dessen Verbreitung in die Neue Welt*, "Abh. d. preuss. Akad. d. Wiss.", 1839, pp. 305-402.
34. *Ibid.*, *Über die geographische Verbreitung der Baumwolle und ihr Verhältniss zur Industrie der Völker alter und neuer Zeit*, 1851, pp. 297-359.
35. *Entdeckungsgeschichte und Fortschritte der wissenschaftlichen Geographie im neunzehnten Jahrhundert* (Berlin, 1902), p. 168.

Some Notes on the Historical Relations Between Geography and Philosophy

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RESPECTING THIS TOPIC, a rough gradation of positions can be worked out. The gradation ranges from professional philosophers who were seriously concerned with geography and whose writings respecting the nature of geography directly reflect their philosophical points of view, through philosophers whose concern with geography is more peripheral and hence more difficult to discern philosophically, through geographers who definitely adopted a philosophical position and whose geographical work presumably reflects such a stance, through geographers whose work reflects much less clearly a specific philosophical conception yet whose work provides textual evidence of some such influence, to geographers whose work presents no textual evidence of any philosophical influence other than in the broad sense that any work is likely to conform largely to the requirements of knowledge of its time. With a few notable exceptions, which I shall indicate later, I am struck by the fact that the conscious adaptation and application of philosophical positions by geographers has been most apparent only recently, especially since the end of World War Two. My paper makes no pretense of being exhaustive; it consists merely of a few illustrations. To extend these notes and develop them fully might require a book. In addition, I mainly pose problems rather than supply solutions.

The obvious case of a professional philosopher concerned with geography is Immanuel Kant. Of most philosophical significance in this regard is his "Introduction" to *Physical Geography*. Although that statement is superficially similar to statements made by his contemporaries, Franz and Busching, most important here is that what Kant may have adapted from them was rethought in characteristic Kantian language. Kant's use of such terms as 'world', 'whole', 'man', 'nature', 'description of nature', 'experience', 'knowledge', 'system', 'idea', 'architectonic', 'propaedeutic', 'outer and inner sense', 'history', 'space' and 'time', indicates that the "Introduction" to *Physical Geography* can be adequately understood only against a background of Kant's mature philosophy. Were it not for the use of the Kantian language, the "Introduction" could be dismissed as of little historical significance.

Especially difficult to deal with is the so-called neo-Kan-

tian tradition, since there never was a neo-Kantian movement in the sense in which one can speak of say positivistic and phenomenological movements that each possess key conceptions held by all their practitioners. The back-to-Kant movement, as mainly an opposition to what was regarded at the time as the excesses of idealism, crude materialism, and evolutionism, began in the 1860s and 1870s, but peaked in the late 19th and early 20th centuries. People came out of Kant in many different ways. Most stressed limited aspects of the Kantian doctrine to the exclusion of other aspects: some stressed specific aspects of epistemology, ethics, or philosophy of science; some tended to stress the more idealistic side while others stressed the more empirical side of Kant. Any many writers extended Kant well beyond the limits of the original Kantian doctrine; for instance, Kuno Fischer in his *A Critique of Kant* interpreted Kant as an ardent evolutionist, which clearly he was not. And the famous neo-Kantian distinction between philosophies and sciences of nature and philosophies and sciences of culture is in one respect not Kantian at all, since Kant stresses that the concept "nature" embraces everything, including one's own body, that can be perceived through the "outer senses". Hence, human society is "natural" and, insofar as it can be dealt with scientifically, can only be understood on the same basis as physical nature is understood. However, for Kant the differences that exist between nature and culture are ethical and not natural, and Kant stresses the primacy of ethics. Perhaps only Hermann Cohen at Marburg can be said to have attempted to create a neo-Kantian system of thought, but his Kantianism was tempered by his Judaism and socialism. There never was a pure neo-Kantianism since Kantian ideas were invariably combined in various ways with other ideas. In brief, the label "neo-Kantian" became a largely nebulous even if not entirely empty label, possessing no common characteristics that identified it specifically. The so-called neo-Kantian influence was extremely diffuse, and strange associations and strong oppositions occurred. For instance, Ernst Mach and Heinrich Hertz were prominent physicists and philosophers of science of the late 19th century who have usually been regarded as standing in rather sharp opposition to one another, yet both were significantly influenced by Kant. Mach's *The Analysis of Sensa-*

tions was influenced by the "empirical" side of the Kantian doctrine, especially by Kant's teaching respecting "sensations" as "appearances". Yet, Mach exerted a major influence on members of the Vienna Circle and on the early logical positivists, especially Rudolph Carnap, who were generally opposed to Kantian ideas, and has often been regarded as a forerunner to 20th century positivism. On the other hand, Hertz, especially in his *Principles of Mechanics*, came out of the a priori side of the Kantian doctrine of science, and stressed that a priori speculation creates the basic principles of science. For Hertz, the most basic statements that can be formulated in science are prescriptive of nature rather than descriptive as they were for Mach. Marxism did not escape neo-Kantian influence. The movement that in time became known as Austro-Marxism was influenced by Kantian ethics, while Lenin, in his *Materialism and Empirio-Criticism*, attacked Kantian epistemology which he regarded as producing an increasingly adverse effect on a number of young Marxist intellectuals in Russia during the first decade of the present century. Charles S. Peirce, regarded by many as the founder of American pragmatism, was significantly influenced by Kant in several respects.¹ Peirce has estimated that he averaged two hours a day for more than three years studying Kant's *Critique of Pure Reason*; in the end, aside from its pragmatic elements, he rejected most of the Kantian teaching. Yet, he was also influenced by Kant's idea of "architectonic", that we create a priori systems or conceptual frameworks within which problems can be located and dealt with comprehensively rather than merely tackled piecemeal. In addition, Peirce took from Kant the idea of the supremacy and immediacy of the ethical over the logical, to the degree that he believed the choice of a logical system presupposed ethics, an idea that extends well beyond Kant's own belief. Here, Peirce foreshadows Frank Ramsey's notion of logic as a "normative" science, an idea that significantly influenced the later Wittgenstein.² Should American pragmatism be regarded as a species of neo-Kantian philosophy?

With respect to geography, there is little direct textual evidence that geographical writings substantively reflected neo-Kantian ideas. I am now inclined to think that in my *Kant's Concept of Geography* I overstated the case for possible neo-Kantian influence on Alfred Hettner. Although Hettner, respecting his classification of geography relative to the rest of science, was clearly influenced by the neo-Kantians Windelband and Rickert,³ and although he was an admirer of the neo-Kantian F.A. Lange's *The History of Materialism and Criticism of its Present importance*, there is virtually no direct textual evidence to suggest that Hettner's substantive work was significantly influenced by neo-Kantian conceptions. Hartshorne, of course, was influenced by Hettner's adaptation of neo-Kantian thinking respecting the classification of the

sciences. And, there is virtually no direct textual evidence to suggest that Vidal de la Blache was significantly influenced by Kantian and neo-Kantian ideas. To substantiate claims of neo-Kantian influence among geographers, specific statements made by geographers would have to be related precisely and analytically to statements made by Kant and neo-Kantian writers; little of this degree of precise analytical scholarship has yet been textually undertaken by geographers.

Francis Bacon's conception of the nature of geography is less clear cut than is Kant's. Yet, that conception relates definitely to his "inductive" and "empirical" philosophy. Geography, in a strict sense confined to the study of the land masses of the globe, and hence exclusive of oceanography, is a part of "natural history", which forms the basis of Bacon's "pyramid of knowledge". "Natural history" collects and organizes facts respecting a wide range of observational phenomena. Comparison among various findings within "natural history" gives rise inductively to the higher order generalizations of "physics". Bacon, in addition, recognized a "civil" or human geography, also to be studied inductively and empirically.⁴

In Descartes, the discernment of any conception of geography is much more elusive, and in fact, strictly speaking, non-existent. Descartes believed ideally in a thoroughgoing unity of science that would eventually produce a single science, physics. Hence, all boundaries created among sciences are artificial, mere props of convenience for weak minds. Yet, in the absence of a thoroughly worked out singular deductive framework, which still eluded Descartes, he often employed distinctions among the different parts of physics. Furthermore, as we descend from the celestial to the terrestrial, nature becomes increasingly complex. "Experiences" and "experiments" become necessary to determine in precisely what manner which lower order hypothetical terrestrial statements, among conflicting alternative "hypotheses", can be deduced from higher order celestial, metaphysical, and even theological statements. However, the Cartesian terms "experience" and "experiment" should not be regarded as empirical in a Baconian sense, since for Descartes there is, strictly speaking, no such thing as sense perception; for Descartes, "sensations" are always "thoughts".⁵ The only major works by Descartes that approximate geography are his *Meteorology*, and Part IV of his *Principles of Philosophy*, entitled "Of the Earth", although these works were regarded by him as parts of physics. Clearly of historical climatological interest, and although often critical of Aristotle, Descartes' *Meteorology* employs that term in its traditional Aristotelian sense, and a similarity exists, despite many crucial substantive differences, among topics discussed by Descartes, and by Aristotle in the original work of the same name. The principles expounded in "Of the Earth" are mainly of a geophysical nature; a few prin-

ciples approximate geographical matters by discussing mountains, plains, seas, air, winds, temperature, springs, and earthquakes.⁶

A more evident example of direct philosophical influence on a mainly geographical text written by a professional philosopher is Albertus Magnus' *The Nature of Places*. Although substantial parts of the book, especially Part III, "A Description of the Regions of the World", are not original, there is clear textual evidence of the influence of the modified Aristotelianism of the late Middle Ages. This is especially evident in Albertus Magnus' sophisticated treatment of the concept "place", which introduces a number of specific and local modifications that go well beyond the original Aristotelian teaching respecting that concept.

One of the most obvious examples of direct philosophical influence on the orientation of a geographical text is Strabo. In his *Geography*, Strabo on several occasions identifies himself with Stoic philosophy, and, although most of the statements in that work cannot be specifically identified as Stoic, Strabo often employs such well known Stoic locutions, among others, as "things both human and divine", and "the virtue of place", and implicitly holds to the traditional Stoic belief in the interdependency of ethics, logic, and physics, yet, in keeping with the middle Stoicism of the peak period of Roman ascendancy, is more inclined to stress the ethical. But many problems remain unresolved respecting the precise meaning and implications of Strabo's identity with Stoic philosophy. The text of Strabo's *Geography* is one of the most uncertain, philologically, that has come down to us from antiquity. Many of the people Strabo specifically referred to as contemporary Stoics who had influenced his thinking are virtually unknown today. Considerable controversy exists at present among classics scholars respecting the possible Aristotelian roots of Stoic conceptions, and the degree to which Aristotle's works were known in Strabo's day.⁷ Furthermore, respecting Posidonius, a major influence on Strabo, there is a growing tendency among classics scholars to regard him as having created a rift within whatever unity Stoic philosophy may have traditionally possessed.⁸ Yet, because of the fragmentary nature of what remains today of Posidonius' work, controversy still exists, and is bound to exist, respecting whether he was a systematic thinker or an eclectic thinker who borrowed rather indiscriminately from the works of his predecessors. Perhaps the best that can be done now, respecting Strabo's Stoic geography, is a comparison of his key utterances with traditionally accepted Stoic beliefs, and with Roman Stoic doctrines extant during his time and still extant today, notably Cicero and Seneca.

The second major thrust of classical geography, largely scientific, is more difficult to identify philosophically. A line of scientific development, with many gaps, can be traced from

the pre-Socratic nature philosophers, through Aristotle and Theophrastus, to such figures as Eratosthenes, Hipparchus, Posidonius, and Marinus of Tyre, which culminates with Ptolemy. Aside from the possible distinction Hecataeus of Miletus made between chorography and chronology, Eratosthenes was the first to have identified geography as a discipline in its own right. Yet, whether or not his conception of the nature of geography was in any way philosophically informed remains uncertain. Eratosthenes is known to have produced philosophical works on ethics and rhetoric, which have not survived. The dominant opinion of antiquity is that Eratosthenes dabbled in philosophy, that such work was eclectic and failed to produce a systematic philosophical framework. The few fragments of his work that have survived, mainly courtesy of Strabo, who was probably not sufficiently equipped to appreciate fully his scientific achievement, do not admit of a clear solution to this problem. The problem of Posidonius has already been mentioned. Although he is known to have written both philosophical and geographical works, the fragments of his work that have come down to us do not permit a clear solution to the problem of the degree to which he managed to work out an integrated system of thought.

Ptolemy's work is largely intact, yet does not reveal a consciously held philosophical stance. He clearly worked within an accepted scientific tradition, and unquestionably accepted its intellectual presuppositions. Although Ptolemy appears to have regarded his map projection as scientifically adequate for its purposes, his attitude toward the empirical limitations of his geographical work reveals a strong empirical thrust to his thinking, reminiscent of an Aristotelian approach. Ptolemy indicates a number of empirical limitations respecting the accurate location of places: the precise mathematical determination of the location of places, as worked out originally by Hipparchus, was little used, could only be employed at very specific and intermittent times, and required further refinement; travellers' accounts of the location of places suffered inevitably from varying degrees of inaccuracy; local people were more reliable witnesses than were those trying to determine locations at a considerable distance, yet the former were often inaccessible; and the earth, both physically and humanly, is subject to change over time.⁹ Yet, Ptolemy's belief in the sublimity of mathematics is clearly more Platonic than Aristotelian.

From time to time, the works of others standing within the scientific tradition of geography reveal little explicit philosophical influence beyond largely inarticulated conformity to the standards of scientific knowledge of the day. Such prominent 16th century geographers as Peter Apanius and Gemma Frisius come to mind in this regard.

Representatives of the English topographic and chorographic tradition of the 16th and early 17th centuries, al-

though accepting unquestioningly some of what passed for current knowledge, also clearly reflected in their writings the growing inductive and empirical approaches of the time, inspired to a considerable degree by the opening up of the world through Renaissance exploration and discovery. Perhaps the earliest philosophical statement is that of Richard Eden. In the "Epistle to the Reader" of his *The First Three English Books on America*, originally published in 1556, Eden reasons from a foundation of Aristotelian "empiricism"; the empirical limitations of Ptolemy are criticized from that perspective. Whether the writings of such prominent representatives as Leland, Camden, Norden, Carew, and Stow reflect Aristotelian "empiricism", or foreshadow the emerging new induction and empiricism of Francis Bacon, since all wrote before Bacon had adequately formulated his position, is unclear from the texts. (But, the detailed articulation of a new philosophical position is, historically, often an after-the-fact accomplishment.) And Richard Hakluyt, insofar as he held a conception of the nature of geography, aside from his primary interest in recording the voyages of exploration and discovery of the English and other nations, clearly held an "historia" or factual inquiry conception of the discipline.

Varenus' *Geographia Generalis* presents philosophical problems. Although there is clear textual evidence that Varenus was influenced, both substantively and conceptually, by Descartes, there is no such clear textual evidence that he was influenced by the induction and empiricism of Francis Bacon. Yet, in my opinion, when Varenus uses the concept "sense perception" he does not do so in the Cartesian sense of "thought", but in a sense that approximates the Baconian usage. If this is so, then it appears to me that Varenus failed to achieve an integrated conception of geography. His "general geography" tends to be Cartesian or rational, while his "special geography" tends to be Baconian or empirical.

To conclude my illustrations, the writings of Alexander von Humboldt and Carl Ritter also present philosophical difficulties. Although Humboldt was originally educated and caught up in the German romantic and idealist traditions, there is clear evidence that at a later date he largely rejected these perspectives as being inadequate and inappropriate for purposes of providing the empirical thrust needed for the doing of science. This rejection is evident in the course of lectures he gave at Berlin in 1827-28,¹⁰ in some passages in his *Kosmos*, and in letters he wrote to Varnhagen von Ense, especially letters 30 and 54.¹¹ (Varnhagen was a highly respected personal confidant, and Humboldt was more frank with him than with almost anyone else. Varnhagen was also largely responsible for getting Humboldt to tone down statements in his *Kosmos* critical of Hegel, Schelling, and their followers.) Humboldt was substantively and conceptually influenced by the French empiricism and materialism of the 19th century -- hence the

roots, along with Kantian influence, of his often meticulous empirical and inductive approach to most scientific problems. Yet, romantic and idealist influences remain in clear view, and are very important. Aside from such occasional romantic utterances as his regard for the universe as "one great whole animated by the breath of life",¹² most important here is the influence of idealism on Humboldt's conception of the doing of science. Increasingly, throughout the 19th century, science had become narrowly analytic in the sense that individual components or elements of reality were being identified and isolated, and studied in ever more minute detail. Essentially, Humboldt, although not rejecting this approach to the doing of science, attached far more importance to an integrated or holistic approach; diverse phenomena should be studied in terms of their mutual influences and interdependencies as they occurred and interacted together in different places of the earth's surface, an approach that approximates what we are today inclined to label "systems" or "ecological". Romanticist influence can also be seen in Humboldt's sophisticated and finely honed conception of the aesthetic appreciation of nature.

There is evidence that the philosophy of nature of Schelling conceptually influenced Ritter, especially the concept of a "whole" understood as an organic or at least quasi-organic "totality".¹³ Ideally, this totality was regarded as a spiritual thing, integrating and synthesizing man and physical nature, with the ethical component of man's nature as primary. Yet, Ritter's writings do not contain extreme statements respecting physical nature comparable to those that sometimes occur in Schelling, and are even more characteristic of some of Schelling's most ardent followers. Ritter's work, especially his sensitively presented regional descriptions, contains a stronger "empirical" element than is typical of the German idealist nature philosophers; however, these regional writings are not strictly speaking "empirical" in the narrow "sense perception" and "direct observation" meanings of that term, since they were almost invariably derived from a careful perusal of secondary source material.

Endnotes

1. Thomas A. Goudge, *The Thought of C.S. Peirce*, Toronto: University of Toronto Press, 1950, pp. 335-37
2. Ludwig Wittgenstein, *Philosophical Investigations*, Oxford: Basil Blackwell, 3rd ed., 1972, I:81, p. 38e.
3. Alfred Hettner, *Die Geographie: Ihre Geschichte, ihr Wesen und ihre Methoden*. Breslau: Ferdinand Hirt, 1927, pp. 112-15.

4. **Francis Bacon**, "Preparative toward Natural and Experimental History: Catalogue of Particular Histories by Titles", in his *The New Organon and Related Writings*, New York: Liberal Arts Press, 1960, p. 286.
5. **Descartes**, *Principles of Philosophy*, Part IV, Principle CLXXXIX.
6. **Ibid.**, Part IV, especially Principles XLIV, XLIX, LIII, LIV, LXIV, LXXVII, LXXIX.
7. For instance, J.M. Rist, *Stoic Philosophy*, Cambridge: Cambridge University Press, 1969, chap. 1, "Aristotle and the Stoic Good"; F.H. Sandbach, *Aristotle and the Stoics*, Cambridge: Cambridge Philological Society, Supplementary Vol. no. 10, 1985; Brad Inwood, *Ethics and Human Action in Early Stoicism*, Oxford: Oxford University, Clarendon Press, 1985, chap. 1. "The Aristotelian Background".
8. For instance, Rist., *op. cit.*, chap. 11, "The Imprint of Posidonius"; I.G. Kidd, "Introduction" to L. Edelstein and I.G. Kidd, eds., *Posidonius, the Fragments*, Cambridge: Cambridge University Press, 1972.
9. **Ptolemy**, *Geography*, Book One, chaps. 4 and 5.
10. **L. Kellner**, *Alexander von Humboldt*, London: Oxford University Press, 1963, pp. 115-117.
11. **Ludmilla Assing**, ed., *Letters of Alexander von Humboldt to Varnhagen von Ense: From 1827 to 1858*, New York: Rudd & Carleton, 1860.
12. **Humboldt**, *Cosmos*, Vol. 1, London: Henry G. Bohn, 1848, p. 3.
13. **Georges Nicolas-Obadia**, "Introduction" to Carl Ritter, *Introduction à la Géographie Générale Comparée*, Paris: Les Belles Lettres, 1974, pp. 7-11.

Musings on the History of Geography and the A.A.G. Directory

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THIS PAPER ORIGINATED as an attempt to compile a mailing list of people interested in the history of geography. I have thought about constructing such a list for quite some time. I already know many of those who publish in the general area of inquiry frequently referred to as geographic thought (especially American), many of whom I correspond with regularly. Yet, I wished to be more systematic and more thorough about compiling the list than noting only those who have published on the history of the discipline. From discussions and correspondence I have discovered that many people (including nongeographers) have an interest in the genre, even though the majority may never publish in the field.

This sort of list would disclose people with kindred interests, and it could serve a variety of purposes: to identify those available to review books in geographic thought; to locate people who might wish to attend and/or participate in a session (at a professional meeting) on the history and philosophy of geography; to facilitate distribution of mailings of interest to historians of geography; to identify those with whom one could send and/or exchange reprints of related articles; and would serve as an eventual "thought directory" of sorts. Long distance collegial contacts are a given in academia today, and such a mailing list could only help facilitate those contacts. It was with these ideas in mind that I began to assemble a list of names.

I decided to begin with the United States. More specifically, I decided to start with those who belong to the Association of American Geographers (A.A.G.). The *AAG Directory, 1987* contains the names of all A.A.G. members as of July 15, 1987 (AAG 1987, p. iii), and all regular members are entitled to list three "topical proficiencies" and three "areal proficiencies." Additionally, all members may belong to three "specialty groups." It was my assumption that those interested in geographic thought would give the "history of geography" -- choice number 23 under topical proficiencies (AAG 1987, p. v) -- as one of their main interests.¹ However, this turned out to be an erroneous assumption. Indeed, as I was soon to discover, the AAG Directory, 1987 is either a highly inaccurate reference tool, or it says a good deal about its membership; perhaps, it is a bit of both.

As I began to go through the *Directory*, looking for

everyone with a T23 after their name (the designation for a topical proficiency in the history of geography), it soon became apparent that my list would uncover an entirely different group from that which I had expected. Moreover, several key factors about the *Directory* and/or the membership were revealed in my search. The remainder of this paper will discuss those findings, followed by the list (of what I shall call T23s) itself.

Initially, I discovered that a significant number of Americans who publish regularly in geographic thought either do not belong to the Association, or they belong but do not give the history of geography as one of their proficiencies. The former condition is easier to explain than the latter. Personal convictions understandably run deep, and I refuse to speculate on the infinite number of reasons why someone in a given discipline chooses not to join his or her national organization.

Many more questions arise, however, about why those who do belong to the A.A.G. have opted not to list this specialization, even when they are prominent in this area. One possible explanation is a confusion between what has traditionally been called the "history and philosophy of geography" (as offered in senior- and graduate-level courses), and "geographic thought."² The former, particularly in recent years, is typified by speculation and armchair rumination on the status and future of the discipline. It probably stems, in part, from what Helen Couclelis calls "the academic elite in geography... who feel they may have reached the end of the road in pursuing the lines of current research traditions, [so] the clouds of philosophy seem to promise a vantage point sufficiently lofty to allow the general layout of the disciplinary maze to be discerned." But, she warns, "[p]hilosophizing in an empirical science is a sure sign of trouble" (Couclelis 1982, p. 105). This sort of endeavor is perhaps best described as quasi-rigorous philosophy, and the historical component is generally an afterthought, not the focus of one's research. It is relatively quick to produce, since there is no fieldwork involved, and, given the subjective nature of philosophical discourse, it is somewhat beyond empirical analysis. The latter enterprise, "geographic thought," is quite different. It necessitates archival fieldwork, so it consumes vast quantities of both time and money. Further, since one is always dealing with historical documents and because the focus of the work is on the past (even if as prelude

to the present), one must be far more cautious and exacting in his or her research. Basically, geographic thought requires historical rigor, while the history and philosophy of geography, all too often, requires little more than familiarity with published material and the desire to speculate broadly.

Is it also possible, perhaps, that there is some kind of stigma, some negative image, tied to geographic thought that might influence those working in that area not to declare it as one of their proficiencies? This is a covert issue, one often denied (except in private conversation). My experience has been that there is an unwritten "rule" that suggests that the history of geography is **not** an appropriate topic for specialization. While working on my dissertation, I recall many occasions when I was queried as to why anyone would want to do doctoral research on Carl Sauer (or kindred topics); likewise, some warned me about the limited employment opportunities if I would pursue such a "bizarre" research focus. Close colleagues have actually asked, "But what's the point of all this? Why don't you do real geography?" An exception seems to exist for those more mature geographers, those who presumably have "paid their dues" already, by working in the more traditional, "acceptable" subfields of geography.³ In essence, many geographers apparently feel that it is something to "dabble in" but not something in which to specialize.

In a related vein, when was the last time a job announcement appeared in the *A.A.G. Newsletter* specifying individuals with this sort of training and/or specialization? Moreover, how many geography departments in the U.S. still require their graduate students to pass a course in the history and philosophy of the field? My sense, particularly following the recent rise in applied geography (and especially G.I.S.) (see AAG 1988, p. 20), is that the number is progressively shrinking and that there is little enthusiasm to see such a course remain. I do not understand this narrow-minded attitude, above all by historical geographers and historical cartographers. One need only look to virtually every other discipline to see that an active interest in one's own academic history is a perfectly legitimate and esteemed undertaking.⁴

I do not know the answers to these questions, nor will I name those who were surprisingly absent from this category of members; certainly those reading this will be as surprised as I was to see many well known names (in geographic thought) missing from the list below. (However, I would greatly appreciate hearing from those individuals, to learn why they did not put this down as a proficiency. I am certainly not trying to judge anyone; I am merely curious.)

It may be that the term "proficiency" has something to do with this state of affairs. It occurred to me that there may be some who publish in the area who, although successful and well known for this type of research, do not consider them-

ves proficient. Yet, if that were true, then this would further confuse the issue since twelve of the seventy-three individuals (nearly 17 percent) who **do** denote the history of geography as one of their topical proficiencies are listed as students (at the time of the *Directory's* printing), and one would not normally expect students to declare any proficiencies -- if one associates students with a period of apprenticeship, thereby precluding proficiency by definition. This may, in fact, be what is happening, as even a cursory glance at the *Directory* will reveal that the vast majority of all student members do not list any proficiencies; nor do most opt to belong to a single specialty group. But, some students do consider themselves "proficient," which raises the question whether the designation (both areal and topical) is meant to suggest actual proficiency, or perhaps mere interest alone. This is how I would, and do, interpret the two categories. Indeed, I find myself frustrated every December when I renew my membership and can list only three topics and three areas! I cannot say whether I am proficient in any topic or area, but I list the ones I do to ensure that I receive the brochures and other relevant notices that different groups send me (by purchasing specific, carefully targeted mailing lists from the A.A.G.) throughout the year. I also list those "proficiencies" that I wish to be identified with, so that others looking through the *Directory* can get a better idea of my interests. I suspect the term "proficiency" is indeed part of the confusion. Perhaps it is a holdover from the days when the A.A.G. was a more selective organization (James and Martin 1978), when only the proficient, well published could become members?

Another possibility that might account for the seeming shortage of people who list an interest in the history of geography, is the fact that our cursory glance at the *Directory* will also detect an interesting, related phenomenon: more people tend to list areal proficiencies than topical proficiencies. Indeed, those who choose to list one or the other typically select areas over topics; likewise, it further appears as though members who list both are inclined to denote a larger number of areal proficiencies than topical proficiencies. Perhaps this is to be expected from a group of (mainly) geographers, since the discipline traditionally has emphasized a focus on places and regions. Thematic specializations came early, and again recently, in our disciplinary development -- the broad mid-section of our history dominated by a regional emphasis -- so this may account for the presumably smaller number of geographers who have less interest in regions than the thematic concerns taking place within those regions. In fact, although I lack the data to support it, my inclination after reviewing the *Directory* is to hypothesize that the younger A.A.G. members, on average, tend to claim more topical proficiencies, whereas the older members, in general, tend to weigh their proficiencies on the place/region side of the spectrum. Additional analysis is needed.

Given the above discussion, I began to question the accuracy of the **Directory**. While looking for T23s, I noticed a variety of other curious facts. I discovered one instance where a member, who had passed away a full year prior to the **Directory** publication date, was still noted as a regular, active member. Also, I found one example where someone had listed two topical proficiencies, by number, where no such numbers existed (see "Steed," AAG 1987, p. 211). This calls to mind the issue of the **Directory** compilation. It is very easy for the individual(s) compiling the **Directory** to mistype, transpose, or key-in the wrong proficiency (or specialty group) codes for any given member. Furthermore, this may account, in part, for those prominent members (in the history of geography) who are missing from the list below. Did they perhaps intend geographic thought to be one of their declared topical proficiencies, only to have the word processor overlook it or perhaps give them a nonrequested proficiency instead? Without asking each one, there is no way to know. Nor is it possible to determine how many, if any, of those listed below are similar "victims" of nonrequested topical proficiencies. But, it does raise important questions about the **Directory** as an accurate research tool.

What follows is a list of those individuals who, according to the A.A.G. **Directory** and given the above remarks and qualifications, gave the history of geography as one of their topical proficiencies. I have used **Directory** appellations and abbreviations throughout, but I did change a few spellings and addresses where I knew they were incorrect. (My sense of A.A.G. procedure, based mainly on my own entry and on other entries where I noticed that two or more people receive mail at the same address yet their mailing addresses are decidedly different, suggests that all names and addresses, etc. are compiled precisely as indicated by members on their renewal forms.) I will not claim that this is a comprehensive listing of everyone who requested T23. I did not ask the A.A.G. for a print-out of these names;⁵ I went through the **Directory** myself looking for each T23. Thus, I may have missed one or two entries. If so, then I would appreciate it if those names are brought to my attention. So, on many counts, this is not a complete list of all A.A.G. members interested in geographic thought. If you are reading this and your name is not listed below, and either you thought you had requested this as one of your topical interests or you are merely interested in getting on a history of geography mailing list, please contact me at the address below. I believe that there is inherent value in such a list, and I anticipate producing an updated, much-expanded version for a future issue of the History of Geography Newsletter.

Finally, names preceded by an asterisk are students (as of July 15, 1987, according to the **Directory**); and, as noted above, this represents approximately 17 percent of all A.A.G. mem-

bers indicating this interest/proficiency. I would assert, irrespective of the topical specialty, that 17 percent represents a very significant figure. In this case, however, given the comparatively meager interest and training in geographic thought one (traditionally) has been exposed to in the U.S., this is indeed a most encouraging figure. To me it signifies that there is a positive, growing concern for this genre of research. Elsewhere I speculated that "the history of geography has experienced tremendous growth in the last 20 years and that its recognition in the future seems fairly secure" (Kenzer 1987, p. 70). Since today's students will be tomorrow's practicing geographers, there is a strong possibility that that postulate may come to pass. I sincerely doubt that student members of the A.A.G. represented 17 percent of those listing an interest in the history of geography even ten years ago. I hope the trend continues.

A.A.G. Members Declaring a Proficiency in the History of Geography

(* = Student Members)

Henry Aay, Geology, Geography & Env. Stud., Calvin College, Grand Rapids MI 49506

Ronald F. Abler, Apt. 913, 2475 Virginia Avenue, NW, Washington, DC 20037

James R. Akerman, 1st Floor, 1464 W. Cullom, Chicago, IL 60613-1310

Frank A. Barrett, 122 Allanford Rd., Agincourt, ON Canada M1T 2N5

Mark Bassin, Geography, Science Hall, University of Wisconsin, Madison, WI 53706

Joanna E. Beck, 640 Throckmorton Avenue, Mill Valley, CA 94941

Vincent R.H. Berdoulay, Dept. of Geography, University of Ottawa, Ottawa, ON Canada K1N 6N5

Walter E. Boettcher, 2515 Baxter Drive, Austin, TX 78745

Dalton K. Brasington, P.O. Box 1415 Charleston, SC 29402

Allen D. Bushong, Dept. of Geography, Univ. of South Carolina, Columbia, SC 29208

Anne Buttmer, Geography, University of Lund, Solvegatan 13, S 223 62, Lund Sweden

Langdon D. Clough, 340 Broadway, Pawtucket, RI 02860

Basil A. Collins, 4506 Indian Ridge Road, Sylvania, OH 43560

Social Darwinism, Pragmatism, and American Geography

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IN 1955, JOHN LEIGHTLY warned against the alleged attempt by William Morris Davis at "monistic and mechanistic... extension of the Darwinian concept of evolution through natural selection into the intellectual and social realm." But to this accusation, typical of the Berkeley school of geography, he added an unusual twist:

It is ironic that precisely in the years when Davis was maturing at Harvard a group of thinkers at the same institution was tearing down the intellectual structure that Herbert Spencer and the social Darwinists had erected (312).

A footnote at the end of this passage cites Philip Wiener's book *Evolution and the Founders of Pragmatism* (1949). Leighly then laments that "uncritical minds" in geography are causing it to lag behind psychology and the other social sciences in noticing that the "assertion" of causal connections between physical and cultural phenomena is "hopelessly weak." (313)

Thus Leighly juxtaposes references to two characteristically American lines of Theory: the biologically-oriented social doctrine called "social Darwinism," which originated in England but became even more popular and more variously elaborated in post-Civil War America; and pragmatism, the practically-oriented school of philosophy and psychology native to the United States, which became very popular in the early years of our century. In this study I seek to evaluate the possible influence of these two lines of theory on American geography. I found very few direct influences, but one important indirect one; a few real connections, and one prominent fictitious one. Let us first clear away the unfortunate fiction.

Mis-Definitions and Fictional Connections

Leighly's 1955 article has now taken on special significance, as the apparent ultimate source of those increasingly frequent references in recent geographical literature to a supposed connection between social Darwinism and environmental determinism. It was later cited by historian-educator Jurgen Herbst in an article claiming that in the early twentieth century, social Darwinism caused "the sickness of American geography;" it was supposedly transmitted by way of Davisian environmental determinism, which "became the geographer's

version of social Darwinism" (1961, 543 and 540). But Herbst merely **declares** this connection between Davis and social Darwinism, taking no steps at all to prove it -- except for citing Leighly, who also offered an assertion rather than a proof.

Geographer Preston James then used Herbst as his sole citation in claiming that social Darwinism may be defined as "ideas of the close dependence of human societies on the physical earth, and of the survival of those societies capable of adjusting to the conditions of the physical environment" (1967, 15). In other words, it is supposedly akin to environmental determinism, which now in addition to the usual charges of being unscientific, should bear the added burden of the guilt associated with what most consider to be the **morally** flawed nature of social Darwinism.

In a 1962 article, Leighly's colleague Carl Sauer denounced Turner's environmentally-oriented frontier thesis as resulting from "the spell of a uniformly determinate course of social evolution as cast by... Herbert Spencer" (1981, 61-62); this suggests Spencer's concept of cosmic evolution rather than social Darwinism in particular, but it leaves a suspicion that Sauer too may have wrongly connected the latter with environmental determinism. It also implies a rather common tendency to lump all natural influences, biological and environmental alike, into a single object of disdain. (Others, by the way, claim that the main influence on Turner was Henry George's idea [1880] that access to public lands inspired hope.)

Probably the most elaborate of the frequent reaffirmations of alleged connections between social Darwinism and environmental determinism -- now considered by some to be well-proven "common knowledge" -- is that by Richard Peet (1985). Concentrating virtually his whole assault on the particularly vulnerable *lebensraum* theorist Friedrich Ratzel and his disciple Ellen Semple, Peet argues that such "legitimation theory," in support of capitalism, racism, and imperialism, is typical of environmental determinism in general, and identifies its social Darwinist character.

Unfortunately for such arguments, the entire edifice of citations and claims that can be traced through Preston James back to Herbst and Leighly collapses like a house of cards, when confronted with only a few basic facts and definitions.

Most importantly, not one of these three early "authorities" on the matter gives even the slightest indication -- at least not in these articles -- that he understands the correct definition or specific content of social Darwinism.

Social Darwinism, as expounded first by Herbert Spencer in England, and later by many others including William Graham Sumner in the United States, is the belief that "the struggle for existence," which supposedly determines the course of evolution in the biological realm, should also be given free rein to operate in civilized society. This doctrine, rather popular in the last half of the nineteenth century, champions *laissez faire* in the realm of economic competition; and some advocates including Spencer (1892) actually approved of suffering and even death among the losers in this competition, believing such a process of weeding out "weaklings" will strengthen the human race as a whole. It is an apologia for a particularly inhumane social and economic policy, focusing on competition between human beings, rather than with nature.

Social Darwinism has been allied with and transformed into various racist doctrines; but it is quite distinct from any form of environmental determinism, despite the fact that certain of its proponents may have believed -- often in common with most others around them -- that environmental influences do operate in society. In fact, one critic of social Darwinism claimed in 1916 that

the primary biological error of social Darwinism is its habit of ignoring the physical universe, of assuming that the cause of progress is not the struggle of man with his environment but rather the struggle of man with man, which in fact yields nothing. (George Nasmyth, quoted in Hofstadter, 1959, 199.)

(Right-wing American politicians, often reflecting social Darwinism to some extent, also tend to ignore natural-environmental factors in favor of "the system," unlike progressives such as George McGovern; see Chappell, 1981, 183.)

Leighly, Herbst, and James evidently fell into a tendency, not unknown among other authors, towards broadening the usual definition of "social Darwinism" to include every possible application of evolutionary ideas to social science. Since it is rather obvious that the central concept in Darwin's theory, that of natural selection, involves direct causal influence of the natural environment, it was easy to suppose carelessly that the most famous application of "Darwinism" to society was one involving environmental determinism. But "social Darwinism," although named after Darwin about 1880, was first promulgated by Herbert Spencer even before the 1859 publication of Darwin's theory, and it assumed Lamarckian mechanisms rather than Darwinian ones (although Spencer later endorsed the latter as well). And in history and social theory, as in daily life, communication is best facilitated by adhering to established definitions. This one, consigning the term "social Dar-

winism" to the realm of economic struggle among humans, has become fixed over a century of frequent usage, right down to recent years (Bannister, 1979; Green, 1981).

Peet, who evidently understands the gist of social Darwinism better than these three, still wants it to encompass too much, including environmental influences. He also gets into a different difficulty by dwelling on Spencer's Lamarckism, and then depending on a source that interprets it in a misleading way: Campbell and Livingstone on neo-Lamarckism in American and British geography (1983). These authors clearly realize that Lamarckian theory posits the inheritance of acquired characters, and that this might occur either by direct action from the natural environment (sometimes called "Geofroism"), or through active striving by the organism. But they seem unaware that Lamarck himself emphasized only the latter process, and that the significance of Lamarckism as opposed to Darwinism has from the very start been in its allowance for adaptive genetic change caused by **deliberate or unconscious action by the individual undergoing the change**, in contrast to selection by external environmental forces from among slowly-accumulating random variations (Simpson, 1968, 67; Ingold, 1986, 226). Accordingly, they go to unrealistic lengths to try to implicate environmental-determinist thinkers with neo-Lamarckism -- ignoring, for example, Huntington's much greater emphasis on Darwinian natural selection, compared to his few mentions of reports in favor of Lamarckism (the Geoffroist variety, at that).

At times it seems that Campbell and Livingstone, and after them Peet, feel that even believing that natural environment establishes conditions and challenges to which humans must adapt qualifies as some sort of environmental determinism -- as if one could escape this accusation only by presuming no interaction between organism and environment at all, where humans are concerned. Such a view would clearly carry the concept of human uniqueness to a ridiculous extreme.

That the primary emphasis in Lamarckism is on willful activity, or use and disuse of parts, by the individual organism, is strongly suggested by the fact that -- as both Campbell and Livingstone (286) and Peet (329) point out -- when contemporary biologists draw analogies to it in the social-scientific realm, they refer to processes involving human initiative, rather than natural forces: namely, the discussions by Peter Medawar and Stephen Jay Gould on the process of transmission of culture by teaching and learning (Gould, 1980, 83). These biologists also make clear that neither they nor the vast majority of their colleagues believe that Lamarckism operates in the biological realm (Gould, 1985), despite occasional claims on its behalf by such dissidents as Australian biologist E.J. Steele (1981). Even if Steele's claims have some validity, as I suspect they might, this would still not negate the enormous significance of natural selection, not to mention the es-

essential role of external environment in setting the challenges which the organism must meet in order to survive, whether or not these responses lead to altered genes. Darwin himself emphasized natural selection while also allowing some scope for Lamarckian processes. Meanwhile, it remains obvious that the **only** evolutionary mechanism almost universally endorsed by biologists today is one with a clear environmental-determinist character: natural selection.

Lamarckism was also present in the thought of many **anti**-Spencerian thinkers around the turn of the present century. Whatever the extent of its validity, it did not detract from the force of the strong refutations of social Darwinism, on both intellectual and moral grounds, issued by these "progressive evolutionists" (Fine, 1979). Such scholars as theologian Walter Rauschenbusch, sociologist Lester Ward, and geographer Peter Kropotkin pointed out that human society exists on a higher plane, defined by special moral imperatives, than do other forms of life subject to no morality beyond "survival of the fittest" (Hofstadter, 1959). Kropotkin even argued convincingly that the whole idea of competition as the engine that drives evolution had been exaggerated and wrongly understood: instead, **within** a species, **cooperation** seems to have greater survival value (1902). In sum, social Darwinism suffered a badly devastated reputation. Numerous reform movements arose during the Progressive era, directed against Spencerian *laissez faire* policies that promoted the growth of insensitive monopoly capitalism. Few have claimed allegiance to social Darwinism **by name** in recent decades, but either it or a close equivalent keeps reappearing up to this day, as a point of view rationalizing right-wing "free enterprise" governmental programs that favor large corporations at the expense of small-scale entrepreneurs and individuals.

Real Trends and Influences in U.S. Geography

Peet's article reveals deep skepticism about **any** applications of biological concepts in social science. This view reflects the thoroughgoing disapproval of "monistic and mechanistic" Darwinism which the Berkeley school disseminated, as revealed in my initial citation from Leighly (1955, 312). It also reflects trends well beyond geography itself. During the first quarter of the twentieth century, the social and human sciences were in the midst of a general retreat from what had obviously become an excessive tendency to apply evolutionary concepts too easily and too comprehensively to social phenomena. The phrase "organic analogy" became a common warning flag, almost a standard logical fallacy. One discipline after another retreated from evolutionary approaches. Among social Darwinism and other theories, those kinds of ideas under suspicion included environmental determinism. This of course had been around in one form or another for over two millennia, but now, thanks to the popular

new Darwinian human geography of Friedrich Ratzel, which included a justification of aggressive national growth in terms of an organic analogy (not in itself an environmental-determinist idea, by the way), had become closely associated with evolutionary theory in many minds.

In the geographical milieu established after 1923 by Carl Sauer at Berkeley, this retreat became the standard battle plan. Inveighing against so-called restrictive definitions of the task of geography, by which was meant mainly the attempt of William Morris Davis to define the field in terms of man-land relationships with emphasis on the causal power of the latter, the Sauer school, marching in step with most other geographers of the time on this issue, proceeded to erect intolerant and durable restrictive barriers of its own, against any serious consideration of environmental influences on culture. Other than to recommend certain pathways, Davis did not himself venture into human geography; but his student Ellsworth Huntington did so in a very ambitious way, starting with his theory of the influence of historic climatic pulsations on the outward migrations of nomads from the grasslands of interior Eurasia (1907; Martin, 1973). As if determined to eradicate every trace of the Davisian program from the profession, Sauer maintained a lifelong opposition to the very concept that climate might have changed during history, and even insisted that the earth's grasslands were caused primarily by human-caused fires, rather than by climate.

Even though it has long since become obvious that Sauer was quite wrong on climatic change (Chappell, 1970a, 1970b, 1971; most of the scholarly world, and also the general literate public, woke up to its importance shortly after the numerous droughts and poor harvests of 1972), and at least exaggerated the impact of humans on the distribution of grasslands, his influence in opposing the general concept of natural-environmental influences remains posthumously in force. Inasmuch as the new global awakening to the importance of ecological problems, which gathered steam during the 1960s and burst into full flower at the start of the 1970s, involved renewed attention to the very kinds of relationships both Davis and Huntington had recommended we keep in mind, it is not surprising that the discipline of geography was left unprepared to make really fundamental contributions to this vital new trend in academic and public life. One simply cannot take the science ecology seriously when blindered against all possible resemblances between human and animal life; it is built directly upon Darwinian principles and insights. Whatever human beings are that is more than merely organic, they are still organic also; organic analogies need to be guarded against only insofar as they are too carelessly constructed. Up to this day, no one has ever produced even a remote approach to an actual proof that influences from natural environment do not operate in human life. The only convincing complaints have been against particular attempts to define them, as Sauer's dis-

sident colleague Rostlund boldly pointed out in 1962.

As to accusations of environmental determinists representing social Darwinism, once we get straight on the definition of this doctrine, we see that there is precious little evidence of it influencing geographical theory in America, or in Britain either. Some might expect to find such evidence in the voluminous writings of Huntington, who elaborated a version of genetic determinism that was much more vulnerable than his work on climatic change and climatic influences, and at times even sounded like racism. Yet actually Huntington argued primarily for the genetic superiority of certain "stocks," or "kiths," comprising sub-groups *within* races, and maintained that genetic superiority of one race over another has never been proven. In his final major work, he wrote as follows in unmitigated disapproval of Nazi racism and geopolitics:

Just as Gobineau... and [Houston] Chamberlain... led the way to the Aryan fallacy as to race, so Mackinder... led the way to the similar fallacy as to the Heartland. Major General Professor Doctor Karl Haushofer, as he likes to be called, became enamored of this fallacy... the German geopolitic founded upon it... [is] an illusion (1945, 201-202 and 206).

This illusion resulted, Huntington goes on, from confusing mere possession of the area with the strength of character and "closely knit democratic social system" (202-203) found among the hardy nomads who long ago used it as a base for conquest. The difficult conditions experienced in "the pastoral mode of life" resulted in selection for qualities of "courage, self-reliance, fidelity to comrades, and power of leadership" (203). But technological change has now reduced the relative strength and mobility of nomadic peoples, and thus also the geopolitical advantages of the Heartland. This analysis well exemplifies Huntington's typical attribution of causation to a combination of cultural, biological, and environmental factors, and it illustrates how he utilized the concepts of natural and cultural selection not to support social Darwinist concepts, but simply to explain how human groups developed their characteristic qualities.

In my search through a varied selection of American geographical literature for favorable comments on social Darwinists and their ideas, actually I found more in the writings of Carl Sauer than anywhere else. They amount to just a few, nearly all contained in a single short speech given in 1951: "Folkways of Social Science" (1968, 100-109). Like Leighly, Sauer here reveals a distaste for the "social evolution" of Spencer and others (104), but he also guardedly wishes "every success" to "the experimentally disciplined neo-Spencerian science of society" (103). And he reserves a few compliments for the most influential social Darwinist in America, William Graham Sumner, noting that Sumner was well trained by his

German professors during his early study in Göttingen, and praising his "relentless dissection of the social institutions of the past and the 'phantasms' of the day" (103). The very title of Sauer's article reflects that of the major work in which Sumner outlined such ideas: *Folkways* (1906). (Sauer's remarks also imply admiration for Sumner's command of fourteen foreign languages [Leyburn, 1968, 407], consistently with the strong emphasis on use of foreign languages in the Berkeley geography program.)

One should take care not to attach too much significance to this approval of Sumner, whose social Darwinism was milder than Spencer's, and who veered away from it in later years, towards his sounder and more lastingly influential survey of folkways and mores (where he introduced to sociology such terms as "in-group" and "ethnocentrism"); and it is the latter that elicits Sauer's endorsement. Still, even such limited praise of social Darwinists may be related to Sauer's distaste for governmental solutions to contemporary problems, which developed during the New Deal, after an earlier period of greater sympathy for progressive causes (FitzSimmons, 1988).

Sauer also had some positive thoughts about Ratzel, but not about his environmental determinism, his *lebensraum* concept, or anything else closely related to social Darwinism. Rather, Sauer credited Ratzel with doing much to introduce ideas of cultural diffusion into geography (Sauer, 1936); and these were then recommended as an *alternative* to environmental determinism.

I have found only two other geographers who at least once made remarks that could be interpreted as favorable to social Darwinist themes, and neither of them was even close to being an environmental determinist. Probably the main influence of social Darwinism in American geography today involves the personal politics of many of its members. But I shall not attempt here the very complex and sensitive task of exploring the ways in which such political views may have affected the scholarship of those who hold them.

Pragmatism and Berkeley Geography

To return to the starting point of this discussion, namely Leighly's 1955 article, it is interesting that the very book by Wiener which he cites in connection with the decline of social Darwinism spins out the story of a very different kind of influence of Darwinian thought in the social and human sciences: that on the pragmatic school of philosophy and psychology of which Charles Sanders Peirce, William James, and John Dewey were the foremost proponents. Since Peirce and James were colleagues on the faculty at Harvard, where together they initiated the philosophy called "pragmatism" in the late 1870s, it is obvious that their school of thought is the one Leighly was referring to as supposedly having refuted "Herbert Spencer and the social Darwinists."

Far from being environmental determinists, these scholars placed heavy emphasis on individual will and creativity, and represented in their special and influential way the broad intellectual reaction against wide-ranging speculative theory and overly mechanistic scientific formulations already mentioned above. As Wiener explains, among Darwinian concepts William James chose to emphasize the element of chance involved in the creation of natural variations, rather than the role of environment in selecting from those variations (103). Elsewhere James emphasized the role of individual initiative as opposed to environment, in shaping the character of great men (1897). It is easy to recognize here features in common with the anti-environmentalist geographical philosophy of Sauer and other geographers of his time.

This empirical, pluralistic, and acausal approach to knowledge differed widely from the speculative system-building of Spencer, and there is no evidence that any leading pragmatist subscribed to the politico-economic doctrines of social Darwinism. Yet neither did any of them play any direct and crucial role in "tearing down the intellectual structure" of social Darwinism -- or for that matter, of environmental determinism -- as Leighly seems to think they did. They simply developed ideas in harmony with the new anti-speculative, anti-mechanistic currents of their time. What pragmatism in particular, and for the most part these wider currents as well, stood for is summarized as follows by James:

A pragmatist... turns away from abstraction and insufficiency, from verbal solutions, from bad *a priori* reasons, from fixed principles, closed systems, and pretended absolutes and origins. He turns towards concreteness and adequacy, towards facts, towards action and towards power. That means the empiricist temper regnant and the rationalist temper sincerely given up. It means the open air and possibilities of nature, as against dogma, artificiality, and the pretence of finality in truth (James, 1907, 162).

It is well known that Carl Sauer was in close touch with another school of thought outside geography which subscribed to just these kinds of preferences: descriptive, anti-evolutionary anthropology of Columbia's Franz Boas and Berkeley's Alfred Kroeber. The question naturally suggests itself, in the light of these similarities and of Leighly's citation, whether Sauer or any of his colleagues were also enthusiastic followers of the pragmatic philosophers. This question has particular interest for me in light of what occurred in my only lengthy conversation with Carl Sauer, in about 1963, while still in graduate school.

Although I was attending another university at the time, I had been told that Sauer welcomed the chance to talk with any student who happened to visit Berkeley. I had not yet published anything in geography, and Sauer most likely was

unaware that I was beginning to develop some sympathy for environmental-causationist theories. His attitude was gracious and helpful. The three main points I remember from the discussion were these: (1) He claimed that while on travels with Sauer, Davis failed to notice landforms which could not be easily explained by his deductive theories. (2) He charged that Huntington tended to remember and cite mainly those facts which fit with his own special theories. And (3) he offered a brief discussion of pragmatic philosophy, in which he revealed his general respect for this intellectual current, and in which he mispronounced the name of Peirce, as if it were "Pierce" instead of sounding like "purse." I do not believe I brought up the subject of pragmatism myself, knowing very little about it at that time, and certainly not suspecting it was connected to geography.

In searching through available writings by Carl Sauer, including all of his more influential theoretical statements, I could not locate any specific references to pragmatism or its proponents. Sauer's son Jonathan and his granddaughter Margaret FitzSimmons, both now teaching at UCLA, each told me (1988) that they could not recall his ever mentioning the subject. Martin Kenzer, who has spent considerable time poring over the Sauer papers, told me he did not remember such a mention either -- although admittedly he did not have it in mind when examining the material. My tentative conclusion is that Carl Sauer, like many other intellectuals educated during the first quarter of the twentieth century when pragmatism was in its heyday, had some limited knowledge of it, and was pleased that it took positions congenial to his own point of view. But the fact that he rarely talked about it, except perhaps to an occasional student interested in matters philosophical, and evidently never published anything about it, suggests that it did not constitute any significant direct influence on his thought. One can suppose that he and his close associate Leighly may have talked about pragmatism now and then, perhaps when Wiener's book was new and intriguing; but the fact that Sauer did not pronounce "Peirce" correctly suggests that he may never have discussed the subject with any philosopher, or even heard a lecture on it.

My search covered major theoretical writings by several other American geographers also, and I found no references to pragmatism in them either. John Frazier, discussing the potential of pragmatism for geography, cites only one example of its influence on methodology that is more than "implicit," and that one, frankly, seems far-fetched (1981, 70-72).

From Pragmatism to Phenomenology

But if there was no significant direct influence of pragmatism on American geography, what about possible indirect influences? One may of course reasonably infer that in a society inculcated with pragmatic attitudes and accustomed

to honoring the likes of Peirce and James as probably the nation's greatest contributions to the world of philosophy, at least something of the spirit of the movement might have rubbed off on some geographers. After all, American society has been one of action rather than contemplation, and the central point of the pragmatic theory of meaning is that truth is to be tested in practice: as James characterizes it, speaking of Peirce's original definition, "our beliefs are really rules for action" (James, 1907, 160-161). But geography tends to be a practically-oriented field of study anyway; so such specific influence from pragmatism might be very difficult to detect.

Another indirect influence is by no means uncertain, and is likely to seem quite surprising to anyone hearing of it for the first time. So it was to me when I learned of it in 1987 by running across a newly-published book by philosopher James Edie: *William James and Phenomenology* (1987b). Amazingly enough, it turns out that this most influential of American pragmatic philosophers, William James, exerted a direct and crucial early influence on the thought of Edmund Husserl, seventeen years his junior, at the very time Husserl was first formulating his phenomenological philosophy. Pragmatic philosophy has therefore helped to shape the one philosophical trend most frequently invoked in support of the recent development of "humanistic" geography.

This relationship was not newly discovered by Edie. Herbert Spiegelberg gave considerable attention to it at least as long ago as 1965 (esp. vol. 1, 65-69 and 111-117). Another discussion of it, carefully comparing and contrasting many facets of the thought of both men, is found in a study of James by John Wild (1969).

It seems that in 1882, the young and unknown Assistant Professor at Harvard, William James, had travelled to Prague in order to interview a few eminent scholars, especially the physicist and positivist philosopher Ernst Mach. In Prague he chanced to meet another philosopher, younger than himself, Carl Stumpf; and the two enjoyed each other's company so much, that their friendship and correspondence became lifelong. Stumpf, termed by Spiegelberg a "pre-phenomenological" theorist, was acquainted with Edmund Husserl, to whom he recommended James's *Principles of Psychology* soon after it appeared in 1890.

Husserl has revealed on several occasions -- in print, in manuscripts, and verbally -- that he found this huge publication, whose subject was as much philosophy as it was psychology, to be of enormous importance in the development of his own thought. In one portion of his now-published diaries, Husserl writes of how impressed he was in seeing here "how a daring and original man did not let himself be held down by tradition." At first, evidently hindered by the language barrier, he read only limited sections of James's *Psychology*; but even at that stage, he found it to be an "absolutely indispensable"

influence (Husserl, quoted in Spiegelberg, vol. 1, 114).

What was the nature of this influence? In general it involved other matters than the pragmatic theory of truth. We can see one important feature of it from the quotation just given: James inspired Husserl to cast aside tradition and strike out on new paths. Seldom has a philosophical movement so thoroughly rejected tradition, as has phenomenology. Devaluing nearly all philosophical theories since Socrates, this renunciation of previous authority emerged in Husserl's student Heidegger in so drastic a form as an avoidance of ordinary language in favor of a plethora of newly-invented terms (the better to avoid biases inherent in the usual words, Heidegger believed). Further down the line of influence, this trend has now reached ridiculous, nihilistic extremes in the "deconstructionism" of Derrida, who engages in such pointless exercises as the "liberation," the rescue from "the police work of a priori grammar," of the phrase "green is or," which Husserl once properly termed nonsense (Caputo, 1987, 104). Of course we must be wary of blaming the originators of a trend, simply because others later extend it to foolish or damaging extremes.

In the late nineteenth century, to speak out against scholarly ideas of a metaphysical or speculative character, as most influential theories of the day were, was to identify oneself with the positivist movement: not so much with Comtean sociological positivism or with Spencerian evolutionary positivism, both of which severely compromised their supposed opposition to metaphysics by constructing large speculative systems of their own, but with the new scientific and philosophical positivism, whose leading representatives included Wilhelm Ostwald, Richard Avenarius, and -- most prominent of all -- Ernst Mach. Although as an important Newtonian physicist, Mach could scarcely renounce theory *per se*, in the tradition of Hume he maintained strict skepticism about any theory not clearly based on empirical fact; he even refused to accept the atomic theory of matter, because in his day there was no device by which an atom could be directly seen. Not unlike both James and Husserl, Mach emphasized personal sensations as the building blocks of knowledge; this led to his being harshly denounced by Lenin, on the very dubious assumption that this amounted to subjective idealism.

Mach is often considered to have been the spiritual father of the logical positivism of the 1920s (Abbagnano, 1967). This most recent positivism, which deals with methods by which empirical sense data could allegedly be built into valid theory by careful use of language and logic, is the type which underlies the methodology of the new quantitative geography, and which has become the target of severe disapproval on the part of humanistically-oriented geographers. Repeated accusations of "positivism" (usually without "logical" attached) against practitioners of quantitative geography, with its

pendant for speculative model-building, have obscured the essentially anti-speculative nature of earlier forms of positivism, of which Mach's so-called "empiriocriticism" is probably the quintessential variety.

All this needs to be understood, in order to see why both Husserl and James (who, as noted above, journeyed to Prague to meet Mach) reflected in important ways the positivistic currents of their day, even though neither resembled very closely those quantitative, hard-science types often termed "positivists" today. James even took religious experience very seriously, trying to explain it in pragmatic terms. Husserl, though obviously attempting to overcome what he saw to be the limitations of positivism, did not disdain its fundamental aims; he even wrote at one point that "it is we who are the genuine positivists" (1931, 86). He realized that phenomenology shares with positivism the goal of grounding all knowledge on sure facts. The phenomenologist, however, goes to a lot more trouble to determine the full range and exact nature of the elemental experience that supplies us with our facts. James's approach was similar, but because he avoided Husserl's step of positing a transcendental consciousness beyond empirical facts, and found all reality in those facts, he is called a "radical empiricist" (Wild, 396).

James also assisted Husserl both in developing his critique of psychologism, and in adapting from Brentano the concept of "intentionality" -- which may, suggests Spiegelberg, have been James's version of it, more than Brentano's (Spiegelberg, vol. 1, 115-117). James's concept of the "stream of thought" (1890) also finds its reflections in Husserl, and through him in his intellectual descendants Heidegger and Sartre. The epistemology of Heidegger, like that of James, is notably pragmatic in character. In avoiding the so-called transcendental reduction, thus remaining closer to the "life-world" of the individual in his search for meaning, James seems closer to these later existentially-oriented thinkers, than to Husserl. Perhaps he was also closer in spirit to two later phenomenologists oriented largely to social theory, whom he also influenced directly: Max Scheler and Alfred Schütz (Spiegelberg, vol. 1, 248; and vol. 2, 631). Schütz in particular has been a strong influence on contemporary humanistic geographers (Ley and Samuels, 1978).

Towards Objectivity and Progressive Applications

In conclusion, let me suggest that there is a serious need to do more than merely recognize these influences and understand the approaches and ideas involved. It is also important to ask whether or not they were correct, in what directions they led, and of what use they might be to us now. We need to keep in mind, for example, that Husserl's phenomenology initiated a trend in philosophy which has moved progressively further

away from the foundationalist approach to knowledge that has characterized nearly all earlier western philosophy, and ever closer to pure subjectivism and relativism. It seems doubtful that James, not to mention Husserl, would consider the ordinary rules of grammar, essential to maintaining interpersonal communication, the sort of *a priori* thought structures we need to be "liberated" from, as does Derrida (who seems not to understand that healthy growth and creativity in language does not involve drastic challenges to its fundamental principles). Here the flight from grand theory and structured thought has gone too far, and it does not seem wise for geography to emulate such models, which one analytic philosopher, referring to Derrida and Foucault, termed dangerous to one's "intellectual hygiene" (cited in Ihde, 1986, 2).

Perhaps it is also time to concede that positivistic approaches have considerable value within their proper sphere, and that they can live side by side with humanistic and phenomenological perspectives; problems occur with each approach not so much in its merely being practiced, as in the occasional issuance of excessive claims as to the comprehensiveness of its significance. Scarcely any solid result in science does not have a mechanistic, reductionist, deductive, and even "positivist" character; the real need is not to heap scorn on these features when we are fortunate enough to obtain knowledge displaying them, but to remain humbly aware of the vast sea of phenomena still awaiting full comprehension. It is in maintaining proper cultivation of the latter -- especially those portions of it with special subjective value to us as human beings -- that the humanistic perspective finds its true and very valuable function. Looking back on positivism as it was understood by most observers in the time of Mach -- an anti-speculative plea for accuracy, rather than a denial of nuances and values -- might facilitate such a re-evaluation.

Both James and Husserl, despite giving special attention to subjective perceptions, retained their faith in foundationalism, or the belief that sure foundations for knowledge do exist; and in the objective nature of reality (although with Husserl, "intersubjective" is the more appropriate term, since he uses "objective" in a special way; see Edie, 1987a). Rather than abandon foundationalism, as Michael Dear is now advising (1988), the chief need seems to be to reaffirm it, to rescue it from the "liberating" pretensions of deconstructionism, which according to its own insistence on lack of sure foundations, cannot have any solid basis for proving that the thought structures it claims to "deconstruct" have in fact been refuted, or that they make any less sense than do its own impertinent language games.

James admitted that his pragmatism, his revolt against the artificial systems of thought of his own era, "does not stand for any special results. It is a method only" (James, 1907, 162). Positivism and phenomenology too have amounted primarily

to methods, rather than "special results." Perhaps it is time again to begin looking for such actual results; to begin abstracting what we can from the objective world in order to construct some beacons pointing to the future, instead of merely wrecking machines hammering away at the past. Of course we want to avoid again reaching a stage at which the most-heralded "results" are so dubious as to give rise to overly skeptical and relativistic intellectual movements, such as have dominated in the past century or so (criticized by Bloom, 1987, who is largely on the mark despite too many gratuitous swipes at left-wing thought). In trying to escape this outcome, we would do well to adopt a humble stance, remembering that human formulations are seldom more than poor copies of reality. We might also aim for objective, rather than absolute, truth: it should be enough to find some facts that we all can agree on, without expecting results marked by the sort of extreme and domineering finality suggested by the word "absolute" (which seems to apply most appropriately to an object we worship, rather than to one we understand -- knowing it is there, but not all about it).

However healthy it is to be wary of unsound speculation, we must also be wary of excess skepticism. Doubts about the concept of causation and the regularity of the physical universe, typical of pragmatic thought, may have been inappropriately extreme even when first voiced. But such views remain at least problematic, whereas some later offshoots from the same trend can be clearly shown to be unsound. I have tried to prove this in the case of attacks against the validity and integrity of environmental causation theory with false claims of complicity with social Darwinism. With such charges properly dismissed, now would seem to be a good time to begin paying more serious attention to the theme of natural-environmental causes. Surely there are ways of avoiding the oversimplifications, exaggerations, and biases found in some earlier efforts of this kind, if only we put our minds more to creating than to debunking. And out of such effort might even come some applications which serve the very same humane and progressive purposes which many recent critics of environmental causation theory have indicated they wish to serve (See, for example, remarks on understanding causes of ideological differences as a key to promoting peace, in Chappell, 1981 and 1986).

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